ON

HEAT AND ELECTRICITY.

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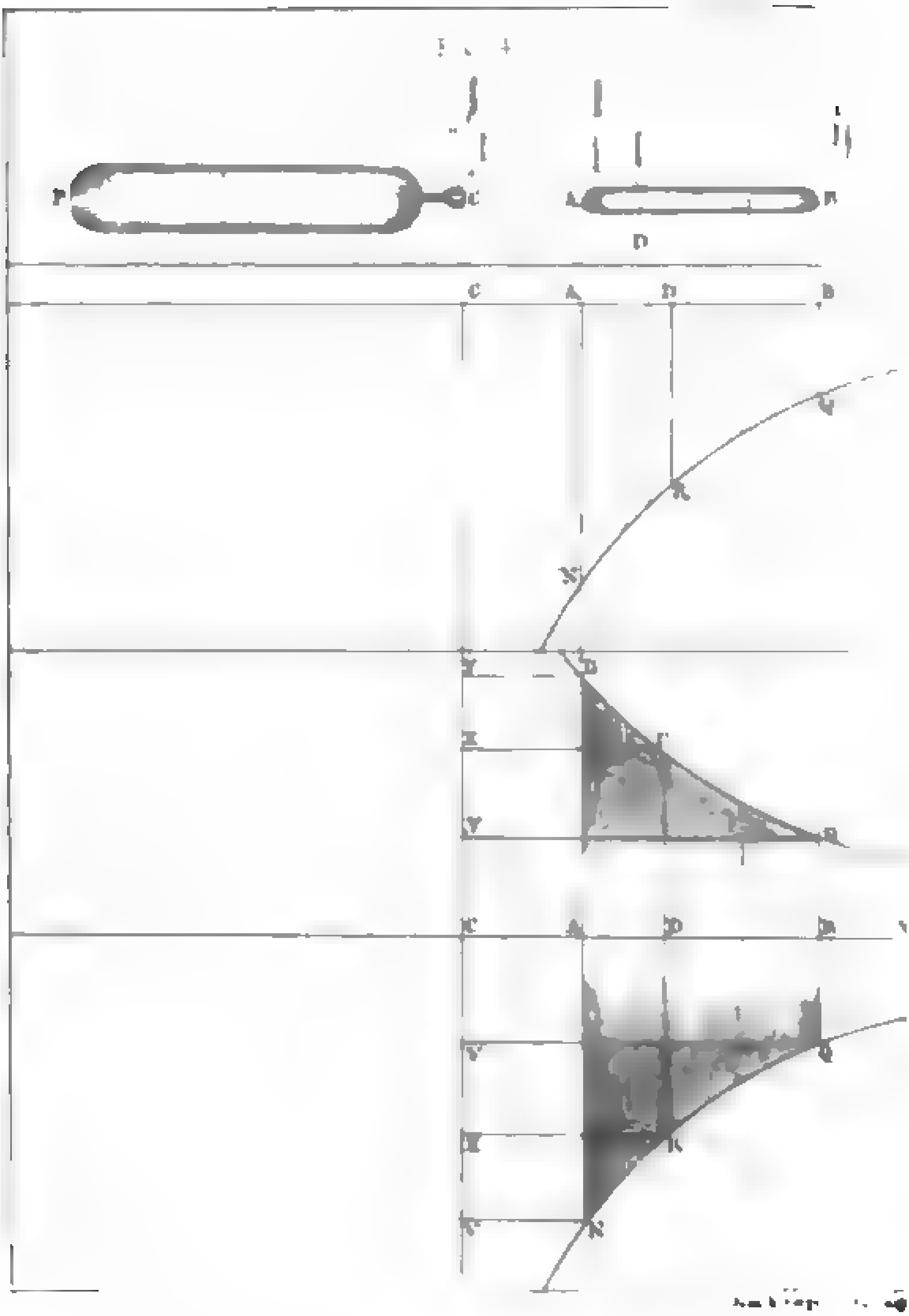

Law of Electric Attraction and Repulsion

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AN

OUTLINE OF THE SCIENCES

OF

HEAT AND ELECTRICITY.

BY

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PREFACE TO THE FIRST EDITION.

The work, which I here lay before the Public, constitutes an abridgement of the Lectures on *Heat* and *Electricity*, which I annually deliver in the College of Glasgow, as a necessary introduction to the elements of Chemistry; and which constitute about a third part of my usual course. Many of the students have been in the habit of applying to me to point out to them a work in the English language in which they could find the different topics which I am in the habit of discussing and illustrating. But I found myself at a loss for any work exactly suited to the objects in view. We have many excellent books on *Heat;* but few or none of them embrace all the topics, which the present state of chemistry renders it necessary for me to introduce into my Lectures. I was therefore under the necessity of mentioning a variety of books, some more, and some less easily accessible, and which, therefore, it was not always in the power of the students to procure when they wanted them.

As for Electricity, we have many books which give a popular view of the science, and not a few which convey very accurate information respecting the mode of making experiments and which, therefore, are of considerable value to the teacher; but we look in vain for any book in the English language which conveys a tolerable notion of the present state of that important science.

It was to remove this inconvenience, which has been long felt and complained of by students, that I have been induced to draw up the present work. It will hereafter serve as a text-book to the students of chemistry in Glasgow College, as it contains an epitome of all the different topics which I discuss, and all the tables to which I refer in the first part of my course; and I flatter myself that the perusal of it will render my lectures hereafter more easily understood and remembered, than when the students had no text-book to have recourse to. I have been at pains to treat of every thing in the plainest and simplest manner; that no difficulties in understanding it might occur even to those students whose previous education has been the most imperfect. I have hopes also that it may be found worthy of the attention of students of chemistry in general, as it contains as accurate a view of the important sciences of *heat* and *electricity* as my knowledge of these subjects enabled me to give.

The present volume may be considered as a new and enlarged edition of the first 172 pages of the first volume of my *System of Chemistry*. The second part, *on Electricity*, is published to fulfil a promise which I gave to the public many years ago. I have withheld it thus long from an unwillingness to encroach beyond what is absolutely necessary on the pecuniary resources of the students. This second part, instead of an abridgement, constitutes, in fact, an extension of my lectures on electricity. In the College of Glasgow the science of electricity belongs, not to the Professor of Chemistry, but to the Professor of Natural Philosophy. This induces me to confine my observations on it to those branches only that are most intimately connected with Chemistry. But I conceive that the connexion between Electricity and Chemistry is now so close, that it is impossible to be master of the latter without being at least acquainted with the principles of the former.

It may be proper to mention, that I am preparing for the

press as complete a view as I can draw up of the *simple substances*, and their primary and secondary compounds, to serve as a text-book for the last two-thirds of my course. This will be followed with as little delay as possible by a work on Mineral Waters, Mineralogy, and Geology. Another work on *Vegetable* Chemistry, and another on *Animal* Chemistry, will complete my plan. For in order to accommodate the students as much as possible, I mean to divide my System of Chemistry into five separate works; the first two of which, consisting of three volumes, will be sufficient for those who are beginning to study the science. Those who wish to devote themselves to the particular departments of chemistry will find the requisite information in the other three works.

PREFACE TO THE SECOND EDITION.

THE discoveries made in HEAT, and especially in ELECTRICITY, since the publication of the former editions of this work, have been so numerous, that a great many additions have been made to both parts of this little volume. No fewer than six additional chapters have been added to ELECTRICITY. This would have swelled the work to a size inconsistent with an elementary treatise. But this has been prevented by reducing the size of the type; which has enabled us to increase the matter in the volume by nearly one-third, without augmenting the bulk of the volume.

It may be right to state that the promises given in the last paragraph of the preface to the first edition of this work, have been nearly all fulfilled. A treatise on *Inorganic Chemistry*, in two volumes; a treatise on *Mineralogy* and *Geology*, in two volumes; and a treatise on *Vegetable Chemistry*, in one large volume, have been already published. Nothing is now wanting to complete the subject, but a volume on *Animal Chemistry*, which I am at present drawing up.

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PART II.—ELECTRICITY.

INTRODUCTION.

One of the earliest attempts at theorizing in chemistry, was to explain the nature of *combustion*. It was this which gave celebrity to Stahl as a chemist, who ascribed *fire* to the escape of phlogiston from the burning body. One of the most constant accompaniments of fire is *heat*. Hence chemists were early led to study the phenomena of *heat*. In Boerhaave's *Chemistry* we find *fire* treated of at great length, and even various speculations and some curious experiments on heat. But in Lemery's *Chemistry*, Neumann's *Chemistry*, and even Wilson's *Chemistry*, published towards the end of the 17th and beginning of the 18th century, no notice whatever is taken of heat. The subject seems to have engaged the attention of Dr Cullen, while lecturing on chemistry in the University of Glasgow; at least it formed a prominent and one of the most interesting parts of Dr Black's lectures, who was his successor both in Glasgow and Edinburgh.

Ever since the time of Dr Black, the doctrine of heat has been treated at considerable length by all teachers of chemistry, at least in Scotland. The subject is important, and as it is not treated by any other professor in our Universities, the Professor of Chemistry has scarcely any other alternative than to explain it to his students. It would be better if a new chair were constituted in each of our Universities, and that it should be the province of him who fills it to explain the principles of *heat*, *light*, *electricity*, and *magnetism*. This chair exists in the Universities on the Continent, at least in France, and this important branch of science is distinguished by the name of *physique*.

The science of chemistry occupies at present so vast a field, that it is impossible to embrace every part of it in a six months'

course. It will be absolutely necessary, before long, to leave out the consideration of heat altogether. The consequence will be, that unless a chair of *physique* be established in our Universities, the alumni educated at them will cease altogether to receive any information respecting one of the most important branches of science.

What has been said of *heat* applies equally to *electricity*, now so closely connected with chemistry, that every chemist must, as a matter of course, make himself acquainted with its principles; but at the same time so extensive, that it is impossible for a Professor of Chemistry to dwell upon it at sufficient length.

The object of the present work was to lay an outline of the most important doctrines of *heat* and *electricity* before the chemical reader. Since the publication of the first edition, in 1830, considerable advances have been made in both these branches of science. I have endeavoured in the present edition to introduce these new facts, and to lay the present state of our knowledge respecting both branches of science before the reader.

The treatise of course is divided into two parts. In the first part I shall treat of *heat*, and in the second of *electricity*.

PART I.—OF HEAT.

Part I.

Meaning of the term heat.

The word *heat* in the English language is used to express two different things. It sometimes signifies a *sensation* excited in our organs, and sometimes a certain *state* of the bodies around us, in consequence of which they excite in us that sensation. The word is used in the first sense when we say that we *feel heat;* and in the second when we say that there is *heat in the fire.* In chemistry, the word *heat* is always employed in the second of these significations. *It denotes that state or condition of a body by which it excites in us the sensation of heat.*

Caloric, what.

When the French chemists contrived the new Chemical Nomenclature in 1787, they thought that it would be advantageous to possess a distinct word for each of these two meanings. Accordingly they restricted the word *heat* to the sensation, and invented the term *caloric* to denote the *cause* of this sensation, or the condition of bodies by which that sensation is produced. This innovation was unnecessary, and it had the effect of fixing down the opinions of chemists to the hypothesis respecting *heat,* adopted by the contrivers of the new nomenclature; namely, that heat is not a *property* of matter, but a *peculiar substance;* although no evidence sufficiently decisive to settle this disputed point has hitherto been advanced.

Heat very communicable.

Heat, whether it be a substance or a quality, is exceedingly communicable from one body to another. If we take a lump of hot iron out of the fire, we cannot prevent it from communicating its heat to all the bodies in its neighbourhood. If a hot body be placed in contact with colder bodies, it communicates heat to them, and continues to do so till all the bodies be reduced to the same temperature. If a cold body be laced in the midst of hot ones, it receives heat from them,

Part I.

and continues to do so till they are all reduced to the same temperature. The common opinion is, that in all such cases the hot body *loses something*, which the cold body imbibes; and this opinion has been universally adopted by all those who have examined the subject more profoundly. According to some philosophers, heat depends upon a *tremor* or *vibration* excited in bodies; according to others, upon an exceedingly *subtile* and *active matter* introduced into their pores; and according to others, upon a *vibration excited in a subtile matter*, which naturally exists in every body, though, except when thus excited, in a state of rest.

Opinions respecting heat.

Bacon was the first person who formally investigated the nature of heat. In his treatise *de forma calidi*, which he offered to the public as a model of the proper method of prosecuting investigations in natural philosophy, he enumerates all the facts respecting heat that were then known; and endeavours, from a careful examination of them, to form a well-founded opinion respecting its cause. The only conclusion which he was able to draw from his premises, was the very general one, that *heat is motion*. In this opinion Lord Bacon has had a great many followers, particularly among the philosophers of Great Britain: but it has been adopted with two different modifications.

Mr Boyle seems to have supposed that this motion is in the small particles of heated bodies, and that it consists in a rapid vibration of these particles. This seems likewise to have been the notion entertained by Sir Isaac Newton, as may be inferred from the queries at the end of his optics. It was also the opinion of Mr Cavendish, and seems likewise to be the view of the subject embraced by Dr Thomas Young.

But the greater number of the French and German chemists of the last century conceived that the motion in which heat consists, is not a tremor of the particles of the hot body, but of the particles of a subtile, highly elastic, and penetrating fluid which is contained among the pores of hot bodies, or interspersed among their particles. This matter they consider as diffused through the whole universe, and as pervading with ease the densest bodies.

All the knowledge which we possess or can ever acquire respecting heat, is that of the different *effects* which it produces upon bodies; we must, therefore, before we can be prepared to form any opinion respecting the nature of heat, take an accurate survey of these effects. Now the general effects of heat are *expansion*, *fluidity*, *vaporization*, *ignition*, *and combustion*. These will form the subject of the five following chapters.*

CHAPTER I.

OF THE FIRST EFFECT OF HEAT, EXPANSION.

In this chapter I shall, in the first place, give an account of *expansion*, or that increase of bulk which takes place when bodies are heated. This will constitute the first section. In the remaining sections of this chapter, I shall treat of certain phenomena of heat, the knowledge of which may be considered as directly derived from the doctrine of expansion.

SECTION I.—OF EXPANSION IN GENERAL.

By expansion is meant an increase in the bulk of bodies, which is observed to take place when they are heated; while there is a corresponding contraction when their temperature is diminished.

Bodies exist in nature in three states; namely, the state of *gases* or *vapours*, the state of *liquids*, and the state of *solids*.

* I have adopted, in treating of heat, the arrangement of Dr Black, from whose lectures I imbibed my first knowledge of this subject. It is very simple, and seems to me better adapted to the present state of our knowledge, than the more elaborate method which I followed in the former editions of my System of Chemistry.

Chap. I.

Common air furnishes a good example of a body in the gaseous state, water of a body in the liquid state, and iron of a body in the solid state. Now gases and vapours expand most when heated, liquids expand less, and solids least of all when subjected to the same increase of temperature.

Expansion of gases.

1. An accurate knowledge of the rate of expansion of gaseous bodies is essential to their investigation. Little progress could be made in determining their properties or constitution till this point was settled; which it was soon after the commencement of the present century, by Mr Dalton and M. Gay-Lussac. The experiments of Mr Dalton were read to the Philosophical Society of Manchester, in October, 1801, and published early in 1802.* To him, therefore, the honour of discovering the law of the dilatation of gaseous bodies must be ascribed; for M. Gay-Lussac† did not publish his dissertation on the expansion of gaseous bodies till more than six months after the appearance of Mr Dalton's paper. The great source of fallacy in experiments on the dilatation of air and gases by heat, is leaving the portion experimented on in contact with moisture; because as the temperature increases, more and more of this moisture is converted into vapour, mixes with the portion of gas under examination, and greatly increases its bulk. Mr Dalton and M. Gay-Lussac, by keeping the gases experimented on dry, were enabled to discover that *all gases* experience the same augmentation of bulk when subjected to the same augmentation of temperature.

Dr Prout determined several years ago the expansion of common air from 32° to 60°. His experiments are not yet published; but he was kind enough to favour me with their general result. Still more lately M. Rudberg has made numerous and very careful experiments upon the expansion of dry air from 32° to 212°.‡ I shall state the results obtained by these experiments.

* Memoirs of the Literary and Philosophical Society of Manchester, vol. v. part 2. p. 595.

† Ann. de Chim. xliii. 137.

‡ Poggendorf's Annalen, xli. 271; and xliv. 119.

Sect. I.

Equable.

2. According to Dalton, when 1000 volumes of air are heated from 32° to 212°, they become 1325 volumes. But he afterwards abandoned this statement and concluded that 1000 volumes of air, when heated from 32° to 212°, become 1376.*

3. According to Gay-Lussac, they become 1375 volumes; while Mr James Crichton of Glasgow, who repeated the experiment with the most minute attention to accuracy, obtained an increase from 1000 to 1374·8 volumes, which differs very little from the estimate of Gay-Lussac.

4. Dr Prout found the weight of 100 cubic inches of air at 32° to be 32·79 grains, while at 60° it weighed only 31·0117 grains. Hence 1000 volumes of air at 32°, when heated to 60°, become 1057·34 volumes. Hence (supposing the expansion to be equable) 1000 volumes, if heated from 32° to 212°, would become 1368·61 volumes.

5. Rudberg's experiments were very numerous. He was at great pains to dry his air thoroughly, and to determine its bulk at 32° and 212°. The result was that 1000 volumes of air at 32°, when heated to 212°, become 1364·57. The least increase in his first set of experiments was 361·7, and the greatest 366·2. In his last experiments the smallest increase was 364·0, and the greatest 366·4.

From the great care with which the experiments of Prout and Rudberg were made, there cannot be any doubt that the expansion of air from 32° to 212°, as stated by Gay-Lussac, is a little too great. I would be disposed to adopt the number given by Prout, because his experiments were made with a degree of care to ensure accuracy which could not be exceeded; but, as his temperature was carried only to 60°, I think it will be right to adopt the result obtained by Rudberg, which is the mean of 20 experiments.

The expansion for 1°, according to Gay-Lussac and Dalton, is $\frac{1}{480}$; according to Prout, $\frac{1}{488\cdot3}$; and according to Rudberg, $\frac{1}{493\cdot7}$. This last number, or $\frac{1}{493}$, which differs very little from it, we shall consider as expressing the correct expansion of dry air for 1° of Fahrenheit's thermometer.

* New System of Chemical Philosophy, i. 10.

Chap. I.

The knowledge of this law supplies us with an easy method of determining what the bulk of a gas would be at a given temperature, provided we know its bulk at any other temperature.*

It is at present the opinion of chemists, that air and all gases experience the same increase of volume by the same increase of temperature, at whatever previous temperature this heat is applied. In other words, that the expansion of all the gases is equable. It is scarcely possible to demonstrate the truth of this opinion experimentally, because we have no means of measuring temperature, except by expansion. But the opinion is founded on very plausible reasons: and if we admit its truth, then the expansibility of all liquids and solids increases with the temperature to which they are subjected.

Gay-Lussac found the expansion of *steam* and of the vapour of *sulphuric ether* the same as that of air. It has been concluded from this, that vapours follow the same law and rate of expansion with gases; and therefore that all elastic fluids agree with each other in this important property.

Mr Dalton has suggested the probability that the expansion of all vapours is the same, reckoning at the same distance from the boiling point of each. Dove has pointed out a curious analogy between the elasticity of those gases which can

* To perform this reduction with accuracy we must attend to the following rules:—

1. If the temperature of the air be 32°, and we wish to know its bulk at a higher temperature, for example, at 60°. Subtract 32° from 494. The remainder is 462. To this number 462, add the degrees above zero indicating the temperature of the air. These are 32 and 60; making 494 and 522. Then say 494 : 522 : : b (bulk of air at 32°) : b' (bulk at 60°.) Let b = 100; then 494 : 522 : : 100 : 105·69 = volume of air at 60°.

2. If the two temperatures be higher than 32° the method is the same. Example, wanted the volume of air at 60° which measures 100 at 50.

462 + 50 = 512   510 : 520 : : 100 : 101·96 = volu.ne
462 + 60 = 522   of the air at 60°.

3. To reduce the volume v of a gas of the temperature t to the lower temperature t', multiply by $\frac{462+t'}{402+t}$. Let t = 80°, t' = 60° then $\frac{462+t'}{402+t}$ $= \frac{522}{542} = \frac{26}{27}$ and $100 \times \frac{26}{27}$ = 96·296 volumes at 60°.

condensed by pressure, and that of steam when subjected the same pressure.* It will be worth while to explain it.

Sect. I.

Analogy of gases and steam.

1. It has been shown by Mr Faraday, that protoxide of ote becomes liquid by the pressure of

50 atmospheres	at 7°·2 Cent.	
44	0°	
Difference	7°·2	

ow, steam balances 50 atmospheres at 265°·9 Cent.

44	258°·2
Difference	7°·7

he difference between the temperatures in these two cases is ·2 and 7°·7, very nearly the same.

2. Carbonic acid becomes liquid by the pressure of

36 atmospheres at	0°
20	—11°·1
Difference	11°·1

Steam balances 36 atmospheres at 226°·4 Cent.

20	214°·7
Difference	11°·7

Here again the differences, 11°·1 and 11°·7, nearly agree.

3. Muriatic acid gas becomes liquid by the pressure of

25 atmospheres at	—3°·9 Cent.
20	—16°·1
Difference	12°·2

Steam balances 25 atmospheres at 226°·3 Cent.

20	214°·7
Difference	11°·6

4. Ammoniacal gas liquefies by the pressure of

6·5 atmospheres at	10° Cent.
5	0
Difference	10

* Poggendorf's Annalen, xxiii. 290.

Chap. I.

Steam balances	6·5 atmospheres at	163°·48 Cent.
	5	153°·08
	Difference	10°·40

From this remarkable coincidence M. Dove infers that reckoning the expansion of gases and vapours equal at the same distance from their boiling points, these gases would become liquid under the pressure of the atmosphere, at the following temperatures:—

	Cent.		Fahrenheit
Protoxide of azote at	—158°	or	—252°·4
Carbonic acid	—146	or	—230·8
Muriatic acid	—130	or	—202
Ammoniacal gas	—53	or	—63·4

The following table gives the elasticity of these and some other gases, in atmospheres at various temperatures. That of steam, at the boiling point of water, being reckoned unity.

Temp. Cent.	Protoxide of Azote.	Carbonic acid.	Muriatic acid.	Ammoniacal gas.	Sulphurous acid.	Vapour of water.
0°	44	36	26·87	5	1·6	0·007
5	47·83	39·21	29·45	5·68	1·87	0·009
10	51·89	42·70	32·23	6·50	2·18	0·012
15	56·26	46·41	35·21	7·27	2·53	0·017
20	60·90	50·41	38·41	8·19	2·92	0·026

Expansion of liquids.

2. No such general law exists respecting the expansibility of liquids. Every liquid has an expansibility peculiarly its own, and which therefore may be called specific. All that can be done therefore, is to determine the degree of expansion which each liquid undergoes, when subjected to a given increase of temperature. The following table exhibits the dilatation of various liquids, from 32° to 212°, supposing the bulk at 32° to be 1.

Alcohol*	0·11000	=	$\frac{1}{9}$†
Nitric acid* (Sp. gr. 1·4) . .	0·11000	=	$\frac{1}{9}$
Whale oil (from 60° to 212°) . .	0·08548	=	$\frac{1}{11\cdot7}$
Fixed oils*	0·08000	=	$\frac{1}{12}$
Sulphuric ether*	0·07000	=	$\frac{1}{14}$

* Dalton's New System of Chemical Philosophy, i. 29.

† The expansion is from 6° to 172°.

Sect. I.

Oil of turpentine	0·07000	=	$\frac{1}{14}$
Sulphuric acid (Sp. gr. 1·85) . .	0·06000	=	$\frac{1}{17}$
Muriatic acid* (Sp. gr. 1·137) .	0·06000	=	$\frac{1}{17}$
Brine, or water saturated with salt*	0·05000	=	$\frac{1}{20}$
Water*	0·04444	=	$\frac{1}{22\cdot5}$
Water from 42°·5 to 212°† . .	0·04393	=	$\frac{1}{22\cdot76}$
Mercury‡	0·018018	=	$\frac{1}{55\cdot5}$
Mercury§	0·01758	=	$\frac{1}{57}$
Mercury‖	0·01680	=	$\frac{1}{59}$
Mercury¶	0·01840	=	$\frac{1}{54\cdot332}$
Mercury**	0·01852	=	$\frac{1}{54}$
Mercury††	0·01872	=	$\frac{1}{53}$
Mercury‡‡	0·02000	=	$\frac{1}{50}$

Liquids differ from gaseous bodies in another circumstance. Their expansibility is not uniform; but the rate of expansion increases with the temperature, and is therefore the greater, the higher the elevation at which a given quantity of heat is added to them. Liquids differ from gaseous bodies in a very remarkable circumstance. The particles of gaseous bodies repel each other, but those of liquids *attract*, as is evident from their collecting together in spherical drops. It has been ascertained that the force of this attraction differs very much in different liquids. For example, it is much greater between the particles of mercury, than between the particles of water. Now these attractions diminish at a great rate, as the distance between the particles of the liquid increase. It is obvious that this attraction between the liquid particles must act as an antagonist to the expansion produced by heat. Hence, the reason that every liquid has an expansion peculiarly its own; and hence also the reason why the rate increases with the temperature. Every increase of temperature weakens the attraction between the particles of the liquid, by increasing their distance from one another, and of course must augment the effect produced by a given increment of heat.

* Dalton's New System of Chemical Philosophy, i. 29.
† Mr Crichton, Annals of Philosophy, (second series,) vii. 244.
‡ Dulong and Petit. § Hällström, Gilbert's Annales, xvii. 107.
‖ General Roy. ¶ Crichton. ** Shuckburgh, Lavoisier and Laplace.
†† Lord Charles Cavendish. ‡‡ Dalton, Chemistry, ii. 289.

Chap. I.

Accurate experiments on the rate of the expansion of few liquids have been hitherto made. The following table exhibits the rate of the dilatation of mercury for the three first *thermic units*,* as determined by the experiments of Dulong and Petit.†

	Dilatation of Mercury.
From 32° to 212° . .	$\frac{1}{5550}$
212 to 392 . .	$\frac{1}{5425}$
392 to 582 . .	$\frac{1}{5300}$

Expansion of water.

Water varies in the rate of its dilatation more than most liquids. The following table exhibits the increase of bulk of 1 of water at various temperatures, from 30° to 212°, as determined by the experiments of Gilpin and Kirwan.

Temp.		Expansion.	Temp.		Expansion.
30°	Gilpin .	0·0020	74°	Gilpin .	·00251
32	G . .	0·0012	79	G . .	·00321
34	G . .	·00006	90	G . .	·00491
39	G . .	·00000	100	G . .	·00692
44	G . .	·00006	102	Kirwan .	·00760
48	G . .	·00018	122	K . .	·01258
49	G . .	·00022	142	K . .	·01833
54	G . .	·00049	162	K . .	·02481
59	G . .	·00086	182	K . .	·03198
64	G . .	·00133	202	K . .	·04005
69	G . .	·00188	212	K . .	·04333‡

If we reckon the temperature at which the water has the greatest specific gravity with Hällström to be 39°·38, the following

* By thermic unit is meant 180°, or the number of degrees between freezing and boiling water.

† Annals of Philosophy, xiii. 118.

‡ The following table exhibits the specific gravity of water at different temperatures, as deduced from the experiments of Captain Kater. It differs somewhat from the table given above.

Temp.	Sp. gravity	Temp.	Sp. gravity.
50° . . .	1·0005	61°	1·0001
51 . . .	1·0005	62	1·0000
52 . . .	1·0005	63	0·9999
53 . . .	1·0004	64	0·9999
54 . . .	1·0004	65	0·9998
55 . . .	1·0004	66	0·9997
56 . .	1·0003	67	0·9996
57 . . .	1·0003	68	0·9996
58 . . .	1·0002	69	0·9995
59 . . .	1·0002	70	0·9994
60 . .	1·0001		

table represents the specific gravity and the volume of that liquid for every centigrade degree, from 0° to 100°, calculated by M. Markiewicz* from the empyrical formula determined by Hällström.† Sect. I.

Temp. Cent.	Specific Gravity.	Volume.	Temp. Cent.	Specific Gravity.	Volume.
0°	[illegible]	1	34	0·9948166	1·0052104
1	1·0000466	0·9999536	35	0·9944706	1·0055601
2	1·0000799	0·9999202	36	0·9941142	1·0059206
[illegible]	1·0001004	0·9998996	37	0·9937480	1·0062903
4	1·00010817	0·9998918	38	0·9933721	1·0066721
4·1	1·00010824	0·99989177	39	0·9929863	1·0070632
5	1·0001032	0·9998968	40	0·9925910	1·0074643
6	1·0000856	0·9999144	41	0·9921958	1·0078656
7	1·0000555	0·9999445	42	0·9917721	1·0083004
8	1·0000129	0·9999872	43	0·9916447	1·0084358
9	0·9999579	1·0000421	44	0·9909138	1·0091694
10	0·9998906	1·0001094	45	0·9904712	1·0096207
11	0·9998112	1·0001888	46	0·9900196	1·0100810
[illegible]	0·9997196	1·0002804	47	0·9895587	1·0105515
13	0·9996160	1·0003841	48	0·9890890	1·0110313
14	0·9995005	1·0004997	[illegible]	0·9887164	1·0114124
15	0·9993731	1·0006273	50	0·9881227	1·0120201
16	0·9992340	1·0007666	51	0·9876255	1·0125295
17	0·9990832	1·0009176	52	0·9871216	1·0130464
18	0·9989207	1·0010805	53	0·9865592	1·0136239
[illegible]	0·9987468	1·0012548	54	0·9860861	1·0141102
20	0·9985615	1·0014406	55	0·9855503	1·0146615
21	0·9983648	1·0016379	56	0·9850173	1·0152106
22	0·9981569	1·0018465	57	0·9844792	1·0157655
23	0·9979379	1·0020664	58	0·9839156	1·0163470
24	0·9977077	1·0022976	59	0·9833526	1·0169292
25	0·9974666	1·0025398	60	0·9827816	1·0175201
26	0·9972146	1·0027932	61	0·9822029	1·0181194
27	0·9969518	1·0030575	62	0·9816263	1·0187176
28	0·9966783	1·0033328	63	0·9810137	1·0193537
29	0·9963941	1·0036189	64	0·9804203	1·0199707
30	0·9960993	1·0039160	65	0·9798108	1·0206052
[illegible]	0·9957941	1·0042337	66	0·9791941	1·0212501
32	0·9956053	1·0044131	67	0·9785699	1·0218994
[illegible]	0·9951527	1·0046699	68	0·9779392	1·0225585

* Poggendorf's Annalen, xix. 135.

† The reader will find Hällström's important paper, containing all his calculations, in Poggendorf's Annalen, i. 129; and his empyrical formula in p. 166 of that volume.

Chap. I.

Temp. Cent.	Specific Gravity.	Volume.	Temp. Cent.	Specific Gravity.	V
69	0·9772999	1·0231250	85	0·9661788	1·03
70	0·9766543	1·0238832	86	0·9655415	1·03
71	0·9760017	1·0245884	87	0·9646791	1·03
72	0·9753297	1·0252943	88	0·9639206	1·03
73	0·9746630	1·0259956	89	0·9631569	1·03
74	0·9740026	1·0266912	90	0·9623877	1·03
75	0·9733229	1·0274083	91	0·9616134	1·03
76	0·9726352	1·0281347	92	0·9608339	1·04
77	0·9719438	1·0288661	93	0·9600492	1·04
78	0·9712446	1·0296066	94	0·9592587	1·04
79	0·9705392	1·0303551	95	0·9584651	1·04
80	0·9698275	1·0311112	96	0·9576658	1·04
81	0·9691096	1·0318750	97	0·9568618	1·04
82	0·9683757	1·0326571	98	0·9560530	1·04
83	0·9676559	1·0334252	99	0·9552889	1·04
84	0·9669160	1·0342159	100	0·9544219	1·04

Next to water alcohol is the liquid whose expansibi been determined with the greatest care. Absolute alc a specific gravity of 0·8062 at the temperature of 32°, ing the specific gravity of water at its point of greatest 1. The following table exhibits the volume of absolute at all temperatures from —100° centigrade, up to 7(grade, supposing the volume at 32° to be unity. It culated by M. Hofmeister, from a very careful set of ments made by professor Muncke of Heidelberg.*

Volumes of pure alcohol.

Temp.	Volume	Temp.	Volume.	Temp.	Vo
—100°	0·9486184612	—84°	0·9479292548	—68°	0·951
99	0·9484472516	83	0·9480260390	67	0·951
98	0·9482938609	82	0·9481385863	66	0·951
97	0·9481578637	81	0·9482687885	65	0·952
96	0·9480392442	80	0·9484105009	64	0·952
95	0·9479378872	79	0·9485686382	63	0·953
94	0·9478536772	78	0·9487440748	62	0·953
93	0·9477864086	77	0·9489336953	61	0·953
92	0·9477362361	76	0·9491383842	60	0·954
91	0·9477027740	75	0·9493580261	59	0·954
90	0·9476859071	74	0·9495925053	58	0·955
89	0·9476857897	73	0·9498417088	57	0·955
88	0·9477020364	72	0·9501055144	56	0·956
87	0·9477346218	71	0·9503838132	55	0·956
86	0·9477834303	70	0·9506764875	54	0·957
85	0·9478483464	69	0·9509834219	53	0·957

* Ann. de Chim. et de Phys. xlix 32.

Volumes of pure alcohol.

Temp.	Volume.	Temp.	Volume.	Temp.	Volume.
—52°	0·9582712034	—11°	0·9892330499	+30°	1·0327133758
51	0·9588150729	10	0·9901769378	31	1·0338631684
50	0·9593710065	9	0·9911281573	32	1·0350155581
49	0·9599388947	8	0·9920865929	33	1·0361704294
48	0·9605186160	7	0·9930521291	[illegible]	1·0373276669
47	0·9611100569	6	0·9940246504	35	1·0384871550
46	0·9617131019	5	0·9950040415	36	1·0396487783
45	0·9623276356	4	0·9959901867	37	1·0408124213
44	0·9629535425	[illegible]	0·9969829706	38	1·0419779685
43	0·9635907072	2	0·9979822778	39	1·0431453045
42	0·9642390141	1	0·9989879927	40	1·0443143138
41	0·9648983477	0	1·0000000000	41	1·0454848809
[illegible]	0·9655685927	+1	1·0010181840	42	1·0466568903
39	0·9662496334	[illegible]	1·0020424294	43	1·0478302266
38	0·9669413546	3	1·0030726206	44	1·0490047743
37	0·9676436405	4	1·0041086423	45	1·0501804179
36	0·9683563759	5	1·0051503788	46	1·0513570419
35	0·9690794452	6	1·0061977149	47	1·0525345309
34	0·9698127330	7	1·0072505348	48	1·0537127693
33	0·9705561237	8	1·0083087233	49	1·0548916418
32	0·9713095020	[illegible]	1·0093721648	[illegible]	1·0560710328
31	0·9720727522	10	1·0104407438	51	1·0572508269
30	0·9728457590	11	1·0115143449	52	1·0584309085
29	0·9736284069	12	1·0125928525	53	1·0596111623
28	0·9744205803	13	1·0136761513	54	1·0607914727
27	0·9752221639	14	1·0147641257	55	1·0619717243
26	0·9760330421	15	1·0158566603	[illegible]	1·0631518015
25	0·9768530995	16	1·0169536396	57	1·0643315890
24	0·9776822206	17	1·0180549481	58	1·0655109711
23	0·9785202899	18	1·0191604703	[illegible]	1·0666898326
22	0·9793671920	19	1·0202700908	60	1·0678680578
21	0·9802228113	20	1·0213836941	61	1·0690455313
20	0·9810870324	21	1·0225011647	62	1·0702221377
19	0·9819597399	22	1·0236223871	63	1·0713977614
18	0·9828408181	23	1·0247472459	64	1·0725722870
17	0·9837301516	24	1·0258756256	65	1·0737455991
16	0·9846276253	25	1·0270074107	66	1·0749175820
15	0·9855331233	26	1·0281424858	67	1·0760881204
14	0·9864465302	27	1·0292807353	68	1·0772570988
13	0·9873677306	28	1·0304220438	69	1·0784244017
12	0·9882966090	29	1·0315662958	70	1·0796899136

Professor Muncke has also examined the dilatability of sulphuret of carbon by heat. The following table exhibits the dilatability calculated from his experiments, from —50° 0° centigrade :—*

* Ann. de Chim. et de Phys. lxiv. 32.

Chap. I.

Volumes of carburet of sulphur.

Temp.	Volume.	Temp.	Volume.	Temp.	Volume.
—50°	0·9478516338	—9°	0·9900068199	+31°	1·0365806688
49	0·9488164414	8	0·9911036176	32	1·0378180146
48	0·9497833230	7	0·9922037873	33	1·0390590232
47	0·9507352857	6	0·9933073362	[illegible]	1·0403037018
46	0·9517263368	5	0·9944142715	35	1·0415520577
45	0·9527064835	4	0·9955246006	36	1·0428040982
44	0·9536867333	3	0·9966393308	37	1·0440598304
43	0·9546700032	2	0·9977554692	38	1·0453192617
42	0·9556565708	1	0·9988780231	39	1·0465823994
41	0·9566461728	0	1·0000000000	40	1·0478492508
40	0·9576390071	+1	1·0011284068	41	1·0491198230
39	0·9586347806	2	1·0022582511	42	1·0503941234
38	0·9596338007	3	1·0033825400	[illegible]	1·0516721592
37	0·9606359746	4	1·0045302908	44	1·0529539377
36	0·9616413097	5	1·0056714808	45	1·0542394662
35	0·9626498131	6	1·0068161473	46	1·0555287520
34	0·9636614922	7	1·0079642874	47	1·0568218022
33	0·9646763542	8	1·0091159086	48	1·0581186243
32	0·9656944063	9	1·0102710180	49	1·0594192254
31	0·9667140560	10	1·0114296229	50	1·0607236129
30	0·9677401104	11	1·0125917331	51	1·0620317939
29	0·9687677707	12	1·0137573485	52	1·0633437758
28	0·9697986624	13	1·0149264836	53	1·0646595659
27	0·9708327748	14	1·0160991434	54	1·0659791713
26	0·9718701205	15	1·0172753350	55	1·0673025994
25	0·9729107076	16	1·0184550658	56	1·0686298575
24	0·9739545430	17	1·0196383430	57	1·0699609528
23	0·9750016341	18	1·0208251738	58	1·0712958926
22	0·9760516000	19	1·0220155657	59	1·0726346841
21	0·9771036121	20	1·0232095258	60	1·0739773347
20	0·9781625136	21	1·0244070613	61	1·0753238515
19	0·9792226008	22	1·0256081797	62	1·0766742419
18	0·9802961780	23	1·0268128880	63	1·0780285132
17	0·9813520555	24	1·0280211937	64	1·0793866726
16	0·9824230304	25	1·0292331040	65	1·0807487273
15	0·9834984371	26	1·0304486261	66	1·0821146847
14	0·9845731559	27	1·0316677673	67	1·0834845520
13	0·9856532030	28	1·0328905349	68	1·0848583365
12	0·9867365658	29	1·0341169362	69	1·0862360454
11	0·9878233112	30	1·0353469784	70	1·0876176861
10	0·9889133869				

Gay-Lussac* has turned his attention to the phenomena of the expansion of liquids. The following table exhibits the result of his researches. He supposes the volume of each of the liquids at its boiling temperature to be 1000. The tabl

* Ann. de Chim. et de Phys. ii. 130.

represents the contractions which each liquid experienced when cooled down every five degrees centigrade below its boiling point. The temperatures at which the different liquids tried boiled were as follows:—

Water . . .	212°
Alcohol . . .	173·14
Sulphuret of carbon .	116·1
Sulphuric ether . .	96·2

Temperature.	Water.	Alcohol.	Sulphuret of carbon.	Ether.
Cent.	Contractions.	Contractions.	Contractions.	Contractions.
0°	0·00	0·00	0·00	0·00
5	3·34	5·55	6·14	8·15
10	6·61	11·43	12·01	16·17
15	10·50	17·51	17·98	24·16
20	13·15	24·34	23·80	31·83
25	16·06	29·15	29·65	39·14
30	18·85	34·74	35·06	46·42
35	21·52	40·28	40·48	52·06
40	24·10	45·68	45·77	58·77
45	26·50	50·85	51·08	65·48
50	28·56	56·02	56·28	72·01
55	30·60	61·01	61·14	78·38
60	32·42	65·96	66·21	
65	34·02	70·74		
70	35·47	75·48		
75	36·70	80·11		

The following table exhibits the degrees marked upon thermometers, filled with different liquids, at the same temperature, as determined by the experiments of De Luc.* The tubes containing these liquids were of glass; but as he does not mention their capacities, nor the value of a degree, the table does not enable us to determine the expansion of the liquids used.

* Recherches sur les Modifications de l'Atmosphere, i. 271.

Chap. I.

Mercury.	Olive oil.	Essential oil of camomile.	Essential oil of thyme.	Alcohol capable of setting fire to gunpowder.	Water saturated with common salt.	Water.
80°	80°	80°	80°	80°	80°	80°
75	74·6	74·7	74·3	73·8	74·1	71·0
70	69·4	69·5	68·8	67·8	68·4	62·0
65	64·4	64·3	63·5	61·9	62·6	53·5
60	59·3	59·1	58·3	56·2	57·1	45·8
55	54·2	53·9	53·3	50·7	51·7	38·5
50	49·2	48·8	48·3	45·3	46·6	32·0
45	44·0	43·6	43·4	40·2	41·2	26·1
40	39·2	38·6	38·4	35·1	36·3	20·5
35	34·2	33·6	33·5	30·3	31·3	15·9
30	29·3	28·7	28·6	25·6	26·5	11·2
25	24·3	23·8	23·8	21·0	21·9	7·3
20	19·3	18·9	19·0	16·5	17·3	4·1
15	14·4	14·1	14·2	12·2	12·8	1·6
10	9·5	9·3	9·4	7·9	8·4	0·2
5	4·7	4·6	4·7	3·9	4·2	0.4
0	0·0	0·0	0·0	0·0	0·0	0·0
—5				—3·9	—4·1	
—10				—7·7	—8·0	

In these thermometers 0 denotes the temperature at which water freezes, 80° the temperature at which it boils.

Law of expansion of liquids.

Mr Emmett has observed, that if the temperatures of liquids be taken in arithmetical progression, the volumes at these temperatures are the logarithms of a certain series of numbers in arithmetical progression; which latter are of course the reciprocals of a series of numbers in harmonical progression.* The following tables will serve to illustrate this law, which constitutes a near approximation to the truth.

MERCURY.

Temp.	Volume.	Log. of	Diff. of numb.
50°	100183	12594·5	
80	—508	12603·98	9·48
110	—813	12612·8	8·82
140	—1119	12621·7	8·9
170	—1424	12630·6	8·9
200	—1730	12639·5	8·9

* Annals of Philosophy, (second series,) viii. 254. The tables are, I believe, founded on a set of experiments which I made many years ago, and which I inserted in an early edition of my Chemistry.

Sect. I.

SULPHURIC ACID.

Temp.	Volume.	Log. of	Diff. of numb.
50°	100000	12591·6	
80	—806	12612·6	21·0
110	—1540	12633·9	21·3
140	—2320	12659·4	25·5
170	—3116	12680·0	20·6
200	—3911	12703·1	23·1

NITRIC ACID.

Temp.	Volume.	Log. of	Diff. of numb.
50°	100000	12589·2	
80	—1530	12633·6	44·4
110	103196	12682·2	48·6
140	—5132	12738·9	46·7

WATER.

Temp.	Volume.	Log. of	Diff. of numb.
50°	100023	12590·0	
70	—197	12594·9	4·9
90	—694	12669·3	4·4

ALCOHOL.

Temp.	Volume.	Log. of	Diff. of numb.
40°	100539	12604·8	
60	—1688	12638·2	33·4
80	—2890	12673·3	35·1
100	—4162	12710·4	37·1

Maximum density of liquids.

It would appear from the investigations of Despretz, Häll-ström, Muncke, &c., that most liquids have a point of maximum density, and that if they be heated or cooled beyond that point, they expand. This point was first observed in water. The phenomena which led to the knowledge of it were first observed by the Florentine academicians. An account of their experiments was published in the *Philosophical Transactions* for 1670.* They filled with water a glass ball, terminating

* Phil. Trans. No. 66, or vol. v. p. 2020. Abridgment, i. 540. See the experiment itself in the English translation of the Memoirs of the Academy del Cimento, p. 77.

Chap. I. in a narrow graduated neck, and plunged it into a mixture of snow and salt. The water started suddenly up into the neck, in consequence of the contraction of the vessel, and slowly subsided again as the cold affected it. After a certain interval it began to rise again, and continued to ascend slowly and equably, till some portion of it shot into ice, when it sprung up at once with the greatest velocity. The attention of the Royal Society was soon afterwards called to this remarkable expansion by Dr Croune, who, in 1683, exhibited an experiment similar to that of the Florentine philosophers, and concluded from it, that water begins to be expanded by cold at a certain temperature above the freezing point. Dr Hooke objected to this conclusion, and ascribed the apparent expansion of the water to the contraction of the vessel in which the experiment was made. This induced them to cool the glass previously in a freezing mixture, and then to fill it with water. The effect, notwithstanding this precaution, was the same as before.* Mr De Luc was the first who attempted to ascertain the exact temperature at which this expansion by cold begins. He placed it at 41°, and estimated the expansion as nearly equal, when water is heated or cooled the same number of degrees above or below 41°. He made his experiments in glass thermometer tubes, and neglected to make the correction necessary for the contraction of the glass; but in a set of experiments by Sir Charles Blagden and Mr Gilpin, made about the year 1790, this correction was attended to. Water was weighed in a glass bottle, at every degree of temperature from 32° to 100°, and its specific gravity ascertained. They fixed the maximum of density at 39°, and found the same expansion very nearly by the same change of temperature either above or below 39°. The following table exhibits the bulk of water at the corresponding degrees on both sides of 39°, according to their experiments.†

* Birche's Hist. of the Royal Society, iv. 253.
† Phil. Trans. 1792, p. 428.

Specific gravity.	Bulk of water.	Temperature.		Bulk of water.	Sp. gravity of do.
	1·00000	39°		1·00000	
1·00000	00	38	40	00	1·00000
0·99999	01	37	41	01	0·99999
0·99998	02	36	42	02	0·99998
0·99996	04	35	43	04	0·99996
0·99994	06	34	44	06	0·99994
0·99991	08	33	45	08	0·99991
0·99988	12	32	46	12	0·99988

Mr Dalton, in a set of experiments published in 1802, obtained nearly the same result as De Luc. He placed the maximum density at 42·5°, not making any correction for the contraction of the glass; and observed, as Blagden had done before him, that the expansion is the same on both sides of the maximum point, when the change of temperature is the same, and continues however low down the water be cooled, provided it be not frozen.*

All these experiments had been made by cooling water in glass vessels; but when the French were forming their new weights and measures, the subject was investigated by Lefebvre-Gineau in a different manner. A determinate bulk of water, at a given temperature, was chosen for the foundation of their weights. To obtain it, a cylinder of copper, about nine French inches long, and as many in diameter, was made, and its bulk measured with the utmost possible exactness. This cylinder was weighed in water of various temperatures. Thus was obtained the weight of a quantity of water equal to the bulk of the cylinder; and this, corrected by the alteration of the bulk of the cylinder itself from heat or cold, gave the density of water at the temperatures tried. The result was, that the density of the water constantly increased till the temperature of 40°, below which

* Manchester Mem. v. 374.

Chap. I. it as constantly diminished.* These experiments seem to have been made about the year 1795. More lately a set of experiments was tried by Hällström exactly in the same way; but he substituted a cylinder of glass for the one of metal. The result which he obtained was the same. The necessary corrections being made, he found the maximum density of water to be 4·1 centigrade, or 39°·38 Fahrenheit.† The subject was afterwards investigated, in a very ingenious set of experiments by Dr Hope. He employed tall cylindrical glass jars, filled with water of different temperatures, and having thermometers at their top and bottom. The result was as follows: 1. When water was at 32°, and exposed to air of 61°, the *bottom* thermometer rose fastest till the water became of 38°, then the top rose fastest. Just the reverse happened when the water was 53°, and exposed to the cold water surrounding the vessel; the *top* thermometer was *highest* till the water cooled down to 40°, then the bottom one was highest. Hence it was inferred, that water when heated towards 40° sunk down, and above 40° rose to the top, and *vice versa*. 2. When a freezing mixture was applied to the top of the glass cylinder (temp. of air 41°,) and continued even for several days, the bottom thermometer never fell below 39°; but when the freezing mixture was applied to the bottom, the top thermometer fell to 34° as soon as the bottom one. Hence it was inferred, that water when cooled below 39° cannot sink, but easily ascends. 3. When the water in the cylinder was at 32°, and warm water applied to the middle of the vessel, the bottom thermometer rose to 39° before the top one was affected; but when the water in the cylinder was at 39°·5, and cold was applied to the middle of the vessel, the top thermometer cooled down to 33° before the bottom one was affected.‡

Count Rumford likewise published a set of experiments,

* Jour. de Phys. xlix. 171; and Huay's Trait de Physique, i. 55 and 181.

† Gilbert's Annalen der Physik, xvii. 207; and Poggendorf's Annalen, i. 129.

‡ See Edin. Trans. vol. vi. The paper was published before October, 1804.

Volumes of pure alcohol.

Sect. 1.

Temp.	Volume.	Temp.	Volume.	Temp.	Volume.
—52°	0·9582712034	—11°	0·9892390490	+30°	1·0327133758
51	0·9588150729	10	0·9901769378	31	1·0338631684
50	0·9593710085	9	0·9911281573	32	1·0350155581
49	0·9599388947	8	0·9920865929	33	1·0361704294
[illegible]	0·9605186160	7	0·9930521291	34	1·0373276669
47	0·9611100569	6	0·9940246504	35	1·0384871550
46	0·9617131019	5	0·9950040415	36	1·0396487789
45	0·9623276356	4	0·9959901867	37	1·0408124213
[illegible]	0·9629535425	3	0·9969829706	38	1·0419779685
43	0·9635907072	2	0·9979822778	39	1·0431453045
42	0·9642390141	1	0·9989879927	40	1·0443143138
41	0·9648983477	0	1·0000000000	41	1·0454848809
40	0·9655685927	+1	1·0010181840	42	1·0466568903
39	0·9662496334	2	1·0020424294	43	1·0478302266
[illegible]	0·9669413546	3	1·0030726206	44	1·0490047743
37	0·9676430405	4	1·0041086423	45	1·0501804179
36	0·9683563759	5	1·0051503788	46	1·0513570419
35	0·9690794452	6	1·0061977149	47	1·0525345309
34	0·9698127330	7	1·0072505348	48	1·0537127693
33	0·9705561237	8	1·0083087233	49	1·0548916418
32	0·9713095020	9	1·0093721648	50	1·0560710328
31	0·9720727522	10	1·0104407438	51	1·0572508269
30	0·9728457590	11	1·0115143449	52	1·0584309085
29	0·9736284069	12	1·0125928525	53	1·0596111623
28	0·9744205803	13	1·0136761513	54	1·0607914727
27	0·9752221639	14	1·0147641257	55	1·0619717243
26	0·9760330421	15	1·0158566603	56	1·0631518015
25	0·9768530995	16	1·0169536396	57	1·0643315890
24	0·9776822206	17	1·0180549481	58	1·0655109711
23	0·9785202899	18	1·0191604703	59	1·0666898326
22	0·9793671920	19	1·0202700908	60	1·0678680578
21	0·9802228113	20	1·0213836941	61	1·0690455313
20	0·9810870324	21	1·0225011647	62	1·0702221377
19	0·9819597399	22	1·0236223871	63	1·0713977614
[illegible]	0·9828408181	23	1·0247472459	64	1·0725722870
17	0·9837301518	24	1·0258756256	65	1·0737455991
16	0·9846276253	[illegible]	1·0270074107	66	1·0749175820
15	0·9855331233	26	1·0281424858	67	1·0760881204
14	0·9864465302	27	1·0292807353	68	1·0772570988
13	0·9873677306	28	1·0304220438	69	1·0784244017
12	0·9882966090	29	1·0315662958	70	1·0796899136

Professor Muncke has also examined the dilatability of bisulphuret of carbon by heat. The following table exhibits that dilatability calculated from his experiments, from —50° to 70° centigrade :—*

* Ann. de Chim. et de Phys. lxiv. 32.

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If we resolve the differential equations $\frac{d\cdot\Delta v}{dt} = 0$ by means of these empyrical formulas, we obtain the following numbers for the degree of greatest density of each liquid:—

1.	Water, . . .	38°·804 Fahrenheit.
2.	Weak Alcohol, . .	—69°·88
3.	Ether, . . .	—38°·08
4.	Petroleum, . . .	—96°·07
5.	Liquid ammonia.	Has no point of maximum density.
6.	Muriatic acid,	Have no point of maximum density.
7.	Nitric acid,	
8.	Oil of Almonds.	
9.	Absolute Alcohol, . .	—140°

10. Bisulphuret of Carbon. Has no point of maximum density; or at least the equation $\frac{d\cdot\Delta v}{dt} = 0$, contains impossible roots.

Despretz has shown experimentally that sea water and all

2. Sea water. $\Delta V = -0{\cdot}00005769938\,t + 0{\cdot}0000050963866\,t^2 - 0{\cdot}00000001873304\,t^3 + 0{\cdot}0000000000617807\,t^4$.

3. Alcohol. $\Delta V = 0{\cdot}0009896860787\,t + 0{\cdot}000003034802828\,t^2 - 0{\cdot}0000000895924\,t^3 + 0{\cdot}0000000000036364\,t^4$.

4. Petroleum. $\Delta V = 0{\cdot}0009885588\,t + 0{\cdot}0000021204\,6\,t^2 - 0{\cdot}0000000267639\,9\,t^3 + 0{\cdot}00000000001950677\,t^4$.

5. Liquid ammonia. $\Delta V = 0{\cdot}000285586\,t + 0{\cdot}000002600199\,t^2 + 0{\cdot}0000000611638\,t^3 - 0{\cdot}00000000104694\,t^4$.

6. Muriatic acid. $\Delta V = 0{\cdot}000566237\,t - 0{\cdot}00000829489\,t^2 + 0{\cdot}00000037084759\,t^3 \quad 0{\cdot}0000000004721563\,t^4$.

7. Nitric acid. $\Delta V = 0{\cdot}0010661285\,t - 0{\cdot}0000016461\,t^2 + 0{\cdot}00000004489136\,t^3 - 0{\cdot}000000000019824\,t^4$.

8. Ether. $\Delta V = 0{\cdot}0015028447\,t + 0{\cdot}00000225214\,t^2 - 0{\cdot}00000015783\,t^3 + 0{\cdot}0000000041466\,t^4$ (1·836).

9. Sulphuric acid. $\Delta V = 0{\cdot}00055161558\,1\,t + 0{\cdot}0000008851987\,t^2 - 0{\cdot}00000008171231\,t^3 + 0{\cdot}0000000000252167\,t^2$.

10. Almond oil. $\Delta V = 0{\cdot}0007445475\,t + 0{\cdot}0000008134379\,t^2 + 0{\cdot}0000002750899\,t^3 - 0{\cdot}000000000015975079\,t^4$.

11. Absolute alcohol. $\Delta V = 0{\cdot}0010151148848\,t + 0{\cdot}0000008884029\,t^2$ $0{\cdot}000000019245568\,t^3$.

12. Bisulphuret of carbon. $\Delta V = 0{\cdot}0011256983898\,t + 0{\cdot}0000017150483\,47\,t^2 + 0{\cdot}0000000012116607\,6569\,t^3$.

t = temperature in centesimal degrees. See Ann. de Chim. et de Phys. [illegible]

aqueous, alcoholic, saline, acid, and alkaline solutions, have a point of greatest density. This point sinks much faster than the freezing point; but both are proportional to the quantity of water in which the other substances are dissolved.*

The explanation of this curious law that water of the temperature of 39° expands when we reduce it to a lower temperature, which is generally adopted, is, that 39° is the lowest point at which water is perfectly fluid. When its temperature falls below this point, its particles begin to assume that new position with respect to each other, which they have when the liquid is converted into ice. Now ice has a lower specific gravity than water; of course the interval between its particles is greater: and the particles begin to assume this greater distance when cooled down below 39°. With respect to the equality of the volume of water at equal distances above and below 39°, it must be accidental, and entirely owing to the diminution which glass experiences in its expansion as the temperature sinks.

Maximum density of brine.

When salt is dissolved in water, the temperature of the greatest density of the water is lowered in proportion to the quantity of salt in solution, so that it speedily sinks below the freezing point of the liquid, in consequence of which the anomaly disappears. Thus water of the specific gravity 1·0100, having about $\frac{1}{70}$th of its weight of common salt dissolved in it, has its point of greatest density at 35°·375, or 4 degrees lower than pure water. Water of the specific gravity 1·0200, or containing $\frac{1}{38}$th of its weight of salt, has its point of greatest density below the freezing point. Water of the specific gravity 1·027, or containing $\frac{1}{27}$th of its weight of salt in solution, has also its point of greatest density below its freezing point. In such liquids, accordingly, the point cannot be observed.†

These facts render the common explanation of the cause of the maximum density of water being at 39°, exceedingly

* Poggendorf's Ann. xli. 66. Erman has published a curious set of experiments and calculations on the specific gravity of sea water and solutions of common salt at different temperatures. See Poggendorf's Ann. vi. 72.

† See Erman's experiments, Ann. de Chim. et de Phys. xxxviii. 287.

Chap. I. doubtful. The experiments of M. Erman on the expar of Rose's fusible metal at different temperatures, is if pos still more inconsistent with it.* What is called Rose's fu metal is an alloy of

2 parts by weight of bismuth,
1 part ——— of lead,
1 part ——— of tin.

It melts at 200°·75, which is considerably lower than the fu point of Newton's fusible metal. Erman has found that specific gravity of this alloy is the greatest at the tempera of 155°·75, and the least at the temperature of 110°·75. at both of these temperatures the metal is solid. It wil worth while to insert here the table of the change of vol which this metal undergoes at different temperatures, as termined by Erman's experiments, on account of the diffe rates at which these alterations take place at different p of the series.

Expansion of fusible metal.

Temp.	Volume.	Temp.	Volume.
32°	1·00000	223°·25	1·01842
43·25	1·00088	234·5	1·01992
54·5	1·00192	245·75	1·02105
65·75	1·00326	257	1·02217
77	1·00443	268·25	1·02289
88·25	1·00681	279·5	1·02395
99·5	1·00803	290·75	1·02529
110·75	1·00830	302	1·02599
122	1·00679	313·25	1·02693
133·25	1·00129	324·5	1·02784
144·5	0·99480	335·75	1·02916
155·75	0·99291	347	1·03072
167	0·99389	358·25	1·03152
178·25		369·5	1·03277
189·5	1·00005	380·75	1·03403
200·75	1·00862	392	1·03495
212	1·01792		

From 32° to 110°·75, the changes of volume are ne proportional to the temperatures. Beyond 110°·75 (wl the volume is a maximum) the metal undergoes a contrac instead of an expansion when heat is added. This cont tion is at first very rapid, but it gradually diminishes to al

* Ann. de Chim. et de Phys. xl. 197.

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°·75, which is the point of greatest density. The volume
178°·25 is nearly the same as at 32°. The volume from
°·75 begins to increase at first very slowly, but the increase
dually augments to 200°·75, which is the point of fusion.
ween 200°·75 and 212°, the increase of volume is very
siderable; but beyond that point the increase is nearly
ilar to what it was between 32° and 110°·75.

Erman has also made a set of experiments to determine the
tation of phosphorus at different temperatures. It has no
nt of maximum density, but continues to dilate as the heat
reases. The following table shows the amount of this dila-
ion.*

Expansion of phosphorus.

Temp.	Volume.	Temp.	Volume.
32°	1·000000	77°	1·008973
38·5	1·001461	88·25	1·011889
43·5	1·002571	96·575	1·014338
49·1	1·004466	100·85	1·046847†
52·25	1·005212	110·3	1·051709
56·75	1·005366	120·425	1·054319
59·9	1·005711	131·48	1·057679
62·15	1·006172	142·25	1·061349
64·175	1·006439	152·375	1·067985
66·75	1·006630	163·625	1·071143
69	1·006953	175·1	1·075070
74·75	1·008504	186·125	1·079086

We see from this table the sudden increase of volume which
he phosphorus undergoes when it becomes liquid. While the
hosphorus is solid, the increase of volume which it sustains
s nearly proportional to the temperature. But liquefaction
roduces a sudden expansion independent of the temperature.
After liquefaction, the dilatations from a given quantity of heat
re much greater than when the phosphorus was solid, but
till these dilatations are proportional to the temperature.

Expansion of solids.

3. Solids are precisely in the same situation with liquids as
far as their expansion by heat is concerned. Their particles
attract each other, with more or less force according to the
solid. This attraction opposes the expanding power of heat.
nce every solid must have a degree of expansion peculiar

* Ann. de Chim. et de Phys. xl. 209.

† At this temperature the phosphorus melted.

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to itself; and the rate of expansion in solids as well as liqu must increase as their temperatures augment.

As glass tubes, filled with mercury, are employed in ord to measure temperature, it becomes an object of great con quence to determine the dilatability of different kinds of gl by heat. Mr James Crichton of Glasgow has examined t dilatability with uncommon care. The following table exhib the results of his experiments.

Expansion of glass.

		1 in volume at 32° becomes at 212°		1 in length at 32° becomes at 212°	
	Sp. gr.	In Decimal.	In Vulg. fr.	In Decimal.	In Vulg f
Colourless glass	3·176	1·002094	[illegible]	1·0006963	[illegible]
Flint glass		1·002422	[illegible]		
Brownish red	3·301	1·002147	[illegible]	1·0007158	[illegible]
Deep blue	3·229	1·002365	[illegible]	1·0007861	[illegible]
Dusky red	3·274	1·002403	[illegible]	1·0008012	[illegible]
Dulong and Petit's		1·002344	[illegible]	1·0007788	[illegible]

Different kinds of glass differ so much from each other th no general rule can be laid down. Lavoisier and Lapla found that it was the less dilatable by heat the more lead contained.† Several determinations will be found in the pr ceding tables, and I shall add some more here. Ramsd found the expansion between 32° and 212° of a solid gla rod 0·0096944, and that of a glass tube 0·0093138. De Lu experiments on the expansion of thermometer and baromet tubes may be seen in the following table.

Temp.	Bulk.	Temp.	Bulk.	Temp.	Bulk.
32°	100000	100°	100023	167°	100056
50	100006	120	100033	190	100069
70	100014	150	100044	212	100083

I shall introduce here the table of the expansion of differe solid bodies from 32° to 212°, as determined by Lavoisier a Laplace, in 1782. The experiments seem to have been ma with very great care. They were supposed to have been lo but have lately been recovered and published by Biot.‡

* The correction pointed out by Mr Crichton in the calculations Dulong and Petit has been made. See Annals of Philosophy, (seco series), vii. 24.

† Biot. Traité de Physique, i. 157. ‡ Traité de Physique, i. 158.

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Substances tried.	Length of a rule at 212° which at 32° is 1,00000000.	Dilatation in vulgar fractions.
Glass of St Gobain	1,00089089	$\frac{1}{1122}$
Glass tube without lead	1,00087572	$\frac{1}{1142}$
Ditto	1,00089760	$\frac{1}{1114}$
Ditto	1,00091751	$\frac{1}{1090}$
English flint glass	1,00081166	$\frac{1}{1248}$
French glass with lead*	1,00087199	$\frac{1}{1147}$
Copper	1,00172244	$\frac{1}{581}$
Copper	1,00171222	$\frac{1}{584}$
Brass	1,00186671	$\frac{1}{535}$
Brass	1,00188971	$\frac{1}{529}$
Hammered iron	1,00122045	$\frac{1}{819}$
Iron wire	1,00123504	$\frac{1}{812}$
Hard steel	1,00107875	$\frac{1}{927}$
Soft steel	1,00107956	$\frac{1}{926}$
Tempered steel	1,00123956	$\frac{1}{807}$
Lead	1,00284836	$\frac{1}{351}$
Malacca tin	1,00193765	$\frac{1}{516}$
Tin from Falmouth	1,00217298	$\frac{1}{462}$
Cupelled silver	1,00190974	$\frac{1}{524}$
Silver, Paris standard	1,00190868	$\frac{1}{524}$
Pure gold	1,00146606	$\frac{1}{682}$
Gold, Paris standard, not softened	1,00155155	$\frac{1}{645}$
Ditto, softened	1,00151361	$\frac{1}{661}$

Experiments of Lavoisier and Laplace.

In the year 1754, Mr Smeaton published a set of experiments on the expansion of different substances, measured by means of a very ingenious instrument of his own invention, described by him in the *Philosophical Transactions* for that year.† The following table shows the expansions which the different substances tried, undergo from 32° to 212°, supposing the original bulk to be unity.

Of Smeaton.

White glass barometer tube	0·00083
Antimony	0·001083
Blistered steel	0·001125
Hard steel	0·001225

* Rudberg says that soda glass expands $\frac{1}{4}$ more than potash glass. See Poggendorf's Annalen, xli. 283. Our English crown glass and also flint glass is made with soda.

† Phil. Trans. 1754, p. 598.

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Iron	0·001258
Bismuth	0·001392
Copper hammered . . .	0·001700
Copper, 8 parts mixed with tin 1 .	0·0018166
Brass, 16 parts with tin 1 . .	0·001908
Brass wire	0·001933
Speculum metal	0·001933
Spelter solder, viz. brass 2, zinc 1 .	0·002058
Fine pewter	0·002283
Grain tin	0·002483
Soft solder, viz. lead 2, tin 1 .	0·002508
Zinc, 8 with tin 1, a little hammered	0·002692
Lead	0·002867
Zinc	0·002942
Zinc hammered ½ an inch per foot .	0·003011

The following table exhibits the dilatations of different substances as determined by General Roy, the accuracy of whose experiments is well known.

General Roy.

Glass tube	0·00077615
Glass rod	0·00080787
Cast-iron prism	0·0011094
Steel rod	0·0011447
Brass scale, supposed from Hamburgh	0·0018554
English plate brass rod . .	0·001875
English plate brass trough .	0·0018928

In the following table I shall give the result of the trials of some other artists and philosophers on the expansion of some other bodies, reckoning as usual the bulk at 32° to be 1. The expansion given is from 32° to 212°.

Steel . . .	0·0011899.	Troughton.
Silver . . .	0·0020826.	Troughton.
Copper . .	0·0019188.	Troughton.
Iron wire . .	0·0014401.	Troughton.
Iron . . .	0·001446.	Hällström.
Platinum . .	0·0009918.	Troughton.
Platinum . .	0·00085655.	Borda.
Palladium . .	0·0010.	Wollaston.

The following table exhibits the expansion of an iron rod in length for different temperatures, as determined by the

periments of Hällström, I believe with considerable accuracy. Sect. I.

Expansion of iron.

Temp.	Length of an iron rod.	1st Diff.	2d Diff.	3d Diff.
40°	0·999632			
22	0·999721	89		
4	0·999811	90	1	
14	0·999904	93	3	2
32	1·000000	96	3	0
50	1·000102	102	6	3
68	1·000211	109	7	1
86	1·000328	117	8	1
104	1·000453	125	8	0
122	1·000588	135	10	2
140	1·000734	146	11	1
158	1·000892	158	12	1
176	1·001063	171	13	1
194	1·001247	184	13	0
212	1·001446	199	15	2

We see from this table that the dilatation of iron increases with the temperature.* Few other experiments have been made to determine the rate at which the expansion of solids increases with the temperature, though there is no reason to doubt that such an increase takes place in all solid bodies. The following table shows the dilatation of iron, copper, and platinum, at 212° and at some higher temperature, as determined by the experiments of Dulong and Petit.

Increased dilatation by heat.

Temp. deduced from dilatation of air.		Dilatation of iron.	Temp. indicated by a thermometer made of iron.		Dilatation of copper.	Temp. by a thermometer made of copper.		Dilatation of platinum	Temp. by a thermometer made of platinum.	
Cent.	Fah.		Cent.	Fah.		Cent.	Fah.		Cent.	Fah.
100°	212°	$\frac{1}{28200}$	100°	212°	$\frac{1}{19400}$	100°	212°	$\frac{1}{37700}$	100°	212°
300	582	$\frac{1}{22700}$	372·6	702·7	$\frac{1}{17700}$	328·8	623·8	$\frac{1}{36300}$	311·6	592·88

* If 1 be the side of the cube constituting unity of volume, d the dilatation of that side for an increase of temperature, the volume of the cube becomes $(1+d)^3$, or $1+3d+3d^2+d^3$. But d in general being very small, $3d^2$ and d^3 may be neglected; so that the new cube may be expressed by $1+3d$. Hence we see that thrice the linear dilatation is very nearly the cubic dilatation.

For the same reason the dilatation of a surface will be expressed by $1+2d$.

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The following linear dilatations have been determined Mr Daniel, when engaged in making experiments with l pyrometer.*

Dimensions which a bar takes whose length at 62° is 1·00000

	At 212° (150°)	At 662° (600°)	At point of Fusion.
Black-lead ware......	1·000244	1·000703	
Wedgewood ware ...	1·000735	1·002995	
Platinum..............	1·000735	1·002995	(1·009926 maxim but not fused.)
Iron (wrought).......	1·000984	1·004483	(1·018378 to the fu point of cast iron.)
Iron (cast)............	1·000893	1·003943	1·016389
Gold	1·001025	1·004238	
Copper..................	1·001430	1·006347	1·024376
Silver....................	1·001626	1·006886	1·020640
Zinc......................	1·002480	1·008527	1·012621
Lead	1·002323	....	1·009072
Tin.......................	1·001472	....	1·003798
Brass. Zinc $\frac{1}{4}$.......	1·001787	1·007207	1·021841
Bronze. Tin $\frac{1}{4}$......	1·001541	1·007053	1·016336
Pewter. Tin $\frac{1}{5}$......	1·001696	....	1·003776
Type Metal	1·001696	....	1·004830

There are various bodies, which, when cooled down t particular point, change their state, and from being liqu become solids. This change is frequently accompanied b diminution of bulk. Thus when olive oil freezes, it sinks the bottom of the unfrozen portion, indicating an increase density or a diminution in bulk. When melted gold or sil is allowed to congeal, it also contracts in its dimensions, diminishes in bulk. It is this diminution which prevents mor from being cast of these metals, and obliges us to stamp But the case is very different with water, and with many otl liquid bodies when they congeal. They experience a cons erable augmentation of bulk in the act of congealing. T consequence is, that the specific gravity of the solid body less than that of the liquid, though its temperature be low These bodies, at that particular temperature at which th change their state, constitute another exception to the gene law of expansion by heat. They increase in bulk when coo down, instead of when heated. This change, however, is

Some bodies expand when they become solid.

* Phil. Trans. 1831, p. 456.

viously owing to the new form which the body assumes, and indicates a new arrangement of the particles, in consequence of which they separate to a greater distance than formerly.

I found, a good many years ago, that ice, supposing it quite free from air bubbles, will remain at rest in any part of a quantity of alcohol diluted with water, till its specific gravity be reduced to 0·92: hence it obviously possesses the specific gravity of 0·92. Messrs Roget and Dumas state the specific gravity of ice to be 0·95.* When ice shoots upon the surface of water, it forms prisms, making angles of 60° and 120° with each other. Mr Dalton has shown that if we consider the particles of water to be spherical and of the same size, and suppose a cubic vessel to be filled with water, the particles at the bottom may be conceived to be placed in regular rows. The second stratum will also constitute regular rows, but each particle will be placed in the intervals between the inferior row, so that each particle will rest upon or in the interval between four particles below it. Let us suppose the cubic box to be drawn into a rhombus, the sides of which make angles of 60° and 120° with each other, the lowest stratum will still consist of the same number of particles as before; but every particle, instead of touching four others, as in the former case, will now touch six others, so that the intervals between the particles will be less than before. But the height of the pile will be increased, every particle in the second stratum resting in the interval of three particles below it. Mr Dalton has shown that the capacities (or the number of particles) in the two vessels will be as 0·707 to 0·750 very nearly, or as 1·00 to 0·942.† Now this is very nearly the difference between the specific gravity of water and ice. Hence it is probable that the diminution of density is owing to such a new arrangement of the particles of water.

As water.

The prodigious force with which water expands in the act of freezing has been long known to philosophers. Glass bottles filled with water are commonly broken in pieces when the

* Annals of Philosophy, (second series,) iii. 392.

† Dalton's New System of Chemical Philosophy, i. 135.

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water freezes. The Florentine academicians burst a brass globe, whose cavity was an inch in diameter, by filling it with water and freezing it. The force necessary for this effect was calculated by Muschenbroeck at 27,720 lbs. But the most complete set of experiments on the expansive force of freezing water are those made by Major Williams at Quebec, and published in the second volume of the *Edinburgh Transactions*.

The same expansion is observed during the crystallization of most of the salts; all of them at least which shoot into prismatic forms. Hence the reason that the glass vessels in which such liquids are left usually break to pieces when the crystals are formed. A number of experiments on this subject have been published by Mr Vauquelin.*

Several of the metals have the property of expanding at the moment of their becoming solid. Reaumur was the first philosopher who examined this point. Of all the metallic bodies that he tried, he found only three that expanded, while all the rest contracted on becoming solid. These three were *cast iron*, *bismuth*, and *antimony*.† Hence the precision with which cast iron takes the impression of the mould.

Cast iron, bismuth, and antimony.

Crystals.

When bodies crystallize, they generally increase in bulk; but when they become solid without any appearance of crystallization, diminution of bulk very frequently accompanies the change. Most of the oils, when they solidify, form very regular spheres. The same thing happens to honey, and to some of the metals, as mercury, which Mr Cavendish has shown from his own experiments, and those of Mr Macnab, to lose about $\frac{1}{23}$d of its bulk in the act of solidification.‡ When sulphuric acid congeals, it does not perceptibly expand, nor does it in the least alter its appearance. Sulphuric acid, of the specific gravity 1·8, may be cooled down in thermometer tubes to —36° before it freezes; and during the whole process it continually contracts. At —36°, or about that temperature,

* Ann. de Chim. xiv. 286.

† Mem. Par. 1726, p. 273. Berthollet's Statique Chimique, ii. 348. The other metals tried by Reaumur were gold, silver, copper, tin, lead, and zinc.

‡ Phil. Trans. 1783, p. 23.

it freezes; but its appearance is so little altered, that I could not satisfy myself whether or not the liquid was frozen till I broke the tube. It was perfectly solid, and displayed no appearance of crystallization. On the other hand, cast iron expands in the act of congealing.* Sect. II.

SECT. II.—OF THE THERMOMETER.

The knowledge of expansion qualifies us for understanding the nature of the *thermometer*, an instrument contrived for measuring the alterations which may take place in the temperatures of bodies.

Discoverer of the thermometer.

The invention of the thermometer, like that of gunpowder, is involved in considerable obscurity. Drebbel, a physician at Alkmaer in Holland, is stated by Boerhaave to have made thermometers about the beginning of the 17th century. Sanctorio, the celebrated founder of statical medicine, who was a professor at Padua, at the commencement of the 17th century, lays claim to the invention of the thermometer.† And this claim is sanctioned by Borelli, who gives us an engraving, together with a description of the original thermometer of Sanctorio.‡ Malpighi, also, who was a professor at Pisa, and the intimate friend of Borelli, ascribes, in his posthumous works, the original invention of the thermometer to Sanctorio.§ These testimonies are sufficient to satisfy us that Sanctorio was the first person who thought of constructing a thermometer, at least in Italy, which was at that period the peculiar seat of the sciences.

First thermometer an air one.

Sanctorio's thermometer was merely a glass tube with a ball blown at the extremity, the open end of which, after the air had been somewhat rarified, was plunged into a coloured liquid. When the air cooled, it resumed its original bulk

* Despretz found that margaric acid, oleic acid, stearic acid, olive oil, stearin, paraffin, and naphthalin, all diminish in volume when they freeze. See Poggendorf's Annalen, xli. 498.

† Com. in Galen. Art. Med. p. 736, 842; as quoted by Dr Martine.

‡ De Motu Animalium, lib. ii. prop. 175.

§ Oper. Post. p. 30. See Martine's Essays, p. 4.

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nearly, and a portion of the coloured liquid rose in the tube. This tube was divided into a number of equal portions, called degrees. When the temperature of this tube was raised, the air in it expanded, and the coloured liquid sank in the tube. When its temperature was lowered, the bulk of the air diminished, and the coloured liquid rose in the tube. The number of degrees which the coloured liquid rose or fell indicated the change of temperature. Thus Sanctorio's instrument was what is called an air thermometer; the changes of temperature being indicated by the alterations in the volume of the air confined in the tube. As the tube was plunged into an open dish filled with coloured liquid, it is evident that the rise and fall of that liquid would be affected not merely by alterations in the temperature, but also by all changes in the density of the atmosphere. When the barometer stood high, the liquid would be more elevated in the tube than when the barometer was low, even supposing no alterations in the temperature.

Improved by the Florentine academicians,

The Florentine academicians, about the middle of the 17th century, made the first improvement on thermometers. They employed a long glass tube, blown at one extremity into a ball, which they filled up to a certain mark in the tube with spirit of wine. The extremity of the tube was then sealed hermetically, by melting it by a blowpipe. The tube was afterwards divided into 100 equal parts, called degrees, by means of small particles of white enamel.* Boyle claims for himself the merit of first introducing such sealed instruments into England. At first, he says, no one would believe that a liquid would expand and contract in a tube hermetically sealed. But he convinced himself of the fact by actual trial, and was still farther satisfied by the sight of a small thermometer, constructed in this way, from Florence.†

and by Fahrenheit.

About the beginning of the 18th century, Mr Fahrenheit, originally a merchant in Dantzic, who, after failing in business, settled at Amsterdam as a thermometer-maker, substi-

* See Waller's Translation of the Essays, &c. of the Academy del Cimento, p. 2.

† Shaw's Boyle, i. 582.

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tuted mercury for spirit of wine, and greatly diminished the size of the tube and the bulb. This rendered the instrument capable of measuring much higher degrees of temperature; for mercury does not boil till raised to a much higher temperature than spirit of wine.

The instrument, as originally made, laboured under a great defect, and many years elapsed before philosophers thought of the proper remedy. No two instruments agreed with each other. The scale of degrees applied to the tube was quite arbitrary. It was differently constructed and differently applied in every thermometer; and experiments made with one could not be usefully compared with those made with another.

How made to correspond with other thermometers.

The most important improvement in these instruments was the contrivance of a method of applying their scales so as to make them agree with each other when exposed to the same temperature, whatever that may be. This was attempted by different methods in different parts of Europe, till at last one was hit upon so superior to all the rest, that it was soon universally employed.

Sir Isaac Newton seems to have first proposed this method;* and Fahrenheit was probably the first thermometer-maker that put it in practice. It is founded on two discoveries made by Dr Hooke: the first in 1664, the second in 1684. It was observed by Dr Hooke that water is changed into ice when cooled down to a particular temperature, and that this temperature remains the same all the time that the water is changing into ice, or the ice into water. If we take a thermometer and plunge it into melting snow, taking care that the ball be completely covered, the quicksilver will be contracted by the cold and descend in the tube. It will at last stop and continue at the same place so long as any considerable part of the snow remains unmelted. If we now mark the part of the tube at which the mercury stopped, and repeat the experiment with the same thermometer, however often, and at places and times however distant, the result will always be the same, the mercury will always descend to the same part of the

* Phil. Trans. 1701, No. 270, p. 824.

Chap. I. tube to which it descended the first time, and will remain s tionary there so long as any considerable part of the sn remains unmelted.* This shows that melting snow is alwa equally cold, or has the power of reducing the thermomet to one steady density, which may be called the *melting sn expansion* of the quicksilver.

The second discovery of Dr Hooke was of a similar natu He found that other things being the same, water always b gins to boil at the same temperature. If, therefore, we ta the thermometer used in the preceding experiments, immer it in boiling water, or surround it with steam, and keep t liquor boiling around it for some time, the mercury will asce to a certain point in the tube, and however long we contin to boil the water, it will ascend no higher. If we mark tl part of the tube to which the mercury rose, and afterwar repeat the experiment ever so often in places of the san height above the surface of the sea, and when the height the barometer is the same, the mercury will always rise to t same point as the first time. Thus boiling water has t power of bringing mercury to another determinate state expansion, which may be called the *boiling water* expansion mercury.†

These two points may be marked upon the tube of a thermometer, by plunging it first into melting snow, and th into boiling water. The distance between them will be ve various in different thermometers, on account of their differe sizes, and the different proportions which the balls and tul bear to each other. But being marked on each instrume

* I have taken no notice in the text of the curious observations lat made at Geneva, that thermometers graduated in the way I have scribed, and kept for a considerable time, at last came to stand perı nently a degree or half a degree above the original freezing point, wl plunged into melting snow. I do not understand the reason of this char and am satisfied that it does not universally take place; for I posses thermometer graduated more than 40 years ago by Mr Crichton of G gow, the freezing point of which is still as accurate as when the inst ment was made.

† The boiling point must be fixed when the barometer stands at a de mined point, usually 29·92 inches.

they will enable us to perceive when the instrument is reduced to the temperature of boiling or freezing water. The distance between these two points may be divided into any number of parts or degrees, taking care always to divide it into the *same* number of degrees in each thermometer, and to number them in the same way in each. The corresponding degrees in the several thermometers, will show the corresponding states of expansion in these different instruments. If we wish to measure other states of expansion *above* or *below* the primary points, we can protract the scale above or below these points by adding to it as many degrees of the same size as the tube will hold, numbering these also in a similar manner in every thermometer. Or we may choose other fixed points which have been found out for these parts of the scale, such as the boiling point of mercury, the melting point of lead, bismuth, or tin, for the higher extremity of the range, and the cold produced by mixing snow or salt, or by pouring dilute nitric acid on snow, for the lower extremity.

As this method was adopted in different countries at different times, and as there never was any connexion between thermometer-makers in different countries, the consequence has been, that the space between the freezing and boiling water points has been divided into a different number of degrees, and this has occasioned a diversity in the thermometers used in different countries, which it will be requisite to explain.

Fahrenheit's thermometer.

Fahrenheit's division is the one which is followed in this country. He did not begin his scale at the melting snow point; but at the temperature produced by mixing snow and common salt, or snow and sal ammoniac. He marked the point at which the thermometer stood when put into such a mixture, zero (0°), and of course made it the beginning of his scale. He then plunged the thermometer into melting snow, and marked the place at which the mercury became stationary in this situation. He divided the distance between these two points into 32 equal parts or degrees. So that upon his scale, the temperature produced by mixing snow and common salt, is marked 0°, while the freezing point of water is marked 32°. He farther protracted the scale with degrees of the same size,

Chap. I. to the top of the tube increasing arithmetically. When a thermometer constructed in this way is plunged into boiling water, the mercury stands at 212°. So that the interval between the freezing and boiling water points, is 180°. To measure greater colds than 0°, a series of degrees of the same size as the others was continued downwards, as far as the tube would admit. These increase arithmetically downwards, and are considered as degrees of cold. They are called degrees below zero, or by prefixing the mark (—) *minus* before them. Thus —20°, means 20° below zero.

The instrument is not now constructed in the way that Fahrenheit followed. All that is necessary, is to find the freezing and boiling water points, to mark the first 32° and the second 212°, and to divide the interval between them into 180 equal parts or degrees. The scale is then protracted upwards and downwards as far as the length of the tube will allow.

Reaumur's thermometer.

In France, numerous experiments on the construction of thermometers were made by Reaumur, during the first half of the 18th century. His thermometer was of spirit of wine. The freezing water point on it was marked 0°, the boiling water point 80°.* De Luc afterwards substituted mercury for spirit of wine, and corrected the inaccuracies under which the original thermometer of Reaumur laboured; but as he did not alter Reaumur's division, the instrument continued always to be called Reaumur's thermometer. It was employed in France before the revolution, and is still used in some parts of Europe. Every degree of Reaumur is equal to 2¼ degrees of Fahrenheit. To convert the degrees of Reaumur to the corresponding number of Fahrenheit, we must multiply by 2·25 and add 32 to the product.

Celsius's.

Celsius, who was professor of astronomy at Upsala, published an account of a new thermometer in the Memoirs of the Stockholm Academy, for 1742.† This thermometer was a

* Reaumur's Memoirs on this subject, will be found in the Volumes of the Memoirs of the Paris Academy of Sciences, for 1730 and 1731.

† Vol. iii. p. 171. Celsius relates experiments showing that the freezing and boiling points of water are constant, other things being equal; but that

rcurial one; the freezing water point was marked 0°, and boiling water point 100°. This thermometer is used in eden. It was adopted by the French at the revolution, der the name of *thermometre centigrade*, and has come into neral use in that country, and in several other parts of urope. Every degree of it is equal to $1\frac{4}{5}$ degrees of Fahnheit. To convert the degrees of Celsius to those of Fahnheit, multiply them by 1·8 and add 32° to the product.

Thermometre centigrade.

The centigrade thermometer is much more convenient for ientific purposes than that graduated according to Fahrenit's division. It is much to be wished that it were generally dopted in this country by men of science.

M. Delisle, a French astronomer, was invited to St Petersburg by the Empress Catherine I. During his residence in that capital from 1726 to 1748, he constructed a thermometer, which has been adopted by the Russians, and is usually known by the name of Delisle's thermometer. In it the numeration begins at the boiling water point, which is considered as the boundary between heat and cold. The numbers from that point increase downwards, and therefore express degrees of contraction. The boiling water point is marked 0°, and the freezing water point —150°. Hence, 5 degrees of Delisle are equal to 6 of Fahrenheit. To convert the degrees of Delisle to those of Fahrenheit, multiply them by 1·2. Subtract the product from 212, if the heat be below boiling water; but if the heat denoted exceed that of boiling water, we must add the product to 212°.

Delisle's.

After thermometers were graduated so as to give results capable of being compared with each other, it became natural to inquire whether the degrees on the scale represented equal increments of heat. The scale of a thermometer divides the increments and diminutions of bulk into a number of small equal parts, that we may see by how many of these parts the bulk of the mercury is increased at one time, or diminished at another. But it remains to be considered whether these equal

Whether the expansion corresponds with the heat.

he boiling water point varies with the height of the barometer. He quotes Dr Martine, Sir Isaac Newton, Fahrenheit, &c. who had preceded him in his observations.

Chap. I. alterations of bulk be produced by equal alterations
Dr Brook Taylor first thought of determining the
perimentally on a lintseed oil thermometer. The
his experiments was published in the Philosophical
tions for 1723.* Dr B. Taylor's mode of experimen
again tried by Dr Black, in 1760, without being awa
had been already anticipated. But Dr Black mad
periments on a mercurial thermometer. De Luc
same experiment nearly about the same time. Dr
did the same thing several years later, and published
in his *Treatise on Heat*.†

To make the experiment successfully, it is neces
the tube of the thermometer be perfectly cylindrica
the length of each degree should vary according to th
the particular part of the tube which it represe
Taylor's experiment was made by mixing together hot
water, by which we can make sure of a knowledge o
differences of temperature, independent of any therm
If we mix together 1 pound of water of the tem
100°, and another pound of the temperature 200°, i
ous that the surplus heat of the hot water will be equa
ed between the two portions of that liquid. One-half
enter the coldest portion and increase its temperatu
the hottest portion will have lost one-half of its surpl
the whole mixture will acquire the same temperatur
is obvious that if the thermometer be an accurate me
heat, this intermediate temperature will be the ari
mean between the temperatures of the two liquids
150°.

The result of the experiments of Black, De
Crawford, is that the expansion of mercury measu
nearly equal increment of heat, as high as their exp
went; that is, to 212°. The variation (if any exi
small, that it may be safely neglected without any
error. But from the experiments of Dulong and Pe
is reason to conclude that the increasing rate of the e

* Vol. xxxii. No. 378, p. 291.

† Experiments and Observations on Animal Heat, p.

mercury becomes sensible at higher temperatures than 212°. Sect. II.
is increased rate becomes sensible when we expose a mercurial and an air thermometer to the same temperature, (correcting the instruments for the expansion of glass;) the following are the points on the scale at which they respectively stand:—

Air therm.		Merc. therm.		Difference.
212°	.	212°	.	0
299·66	.	302	.	2·33
386·69	.	392	.	5·31
473·09	.	482	.	8·91
558·86	.	572	.	13·14
662	.	680	.	18

We see from this table (the experiments were made by Dulong and Petit), that the boiling point of mercury, measured by its own expansions, is 680°; but measured by the expansion of air, 662°. Hence the increment of expansion over that of heat at that temperature, amounts to 18°, or (nearly) $\frac{1}{37}$ of the whole.

Error corrected by the expansion of the tube.

If we plunge a common well graduated thermometer into boiling mercury, it stands, according to the observations of Mr Crichton, at 660°: so that the expansion of the glass is equivalent to 20°. It therefore almost exactly counteracts the increase of the rate of the expansion of the mercury. The consequence of this fortunate coincidence is, that an accurately graduated mercurial glass thermometer is an accurate measurer of the increase of temperature as high as the boiling point of mercury, or to 662°.

From the different methods followed by philosophical instrument-makers in determining the boiling point, it was found that thermometers very seldom agree with each other, and that they often deviated several degrees from the truth. This induced Mr Cavendish to suggest to the Royal Society the importance of publishing rules for constructing these very useful instruments. A committee of the Society was accordingly appointed to consider the subject. This committee published

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a most valuable set of directions, which may be consulted in the *Philosophical Transactions*.* The most important of these directions is, to expose the whole of the tube as well as the ball of the thermometer to steam, when the boiling water point is to be determined. They recommend this to be done when the barometer stands at 29·8 inches. When a thermometer is to be employed as an instrument to determine the temperature of any place where it is suspended, it is obvious that it never can be accurate unless this mode of graduation be followed. But when we employ it in the laboratory to determine the temperature of water or any substance, it would be inconvenient and often impossible to plunge the whole instrument into the body whose temperature we wish to determine. In general, we can only apply or plunge the bulb into it. Hence it is better to graduate such instruments simply by plunging the bulb into the freezing and boiling water, in order to determine the freezing and boiling water points.

Thermometer should be small.

The thermometer merely indicates the change of temperature which it undergoes itself, when applied to a hot or cold body. It will not give us a correct idea of the temperature of another body into which we plunge it, unless it bears a very small ratio in point of size to that of the body under examination. We must wait for some time till the thermometer become stationary before we draw our conclusion. If the temperature of the body examined be undergoing alteration, (either augmenting or diminishing,) the size of the thermometer applied ought to be very small, that it may acquire the temperature of the body to which it is applied as rapidly as possible. Indeed, if the thermometer be of a considerable size, it will never indicate the maximum temperature of a body, provided that temperature be of short duration. I suspended a very large and a very small thermometer near each other in a north exposure, and shaded from the sun, to determine the summer temperature of Glasgow; and I almost constantly found the small thermometer a degree or two higher than the

* Phil. Trans. 1777, p. 816.

large one, about the time of the day when the temperature was highest, and a degree or two lower when the temperature was coldest. The mean temperature of the day indicated by each thermometer corresponded, but the extremes differed several degrees. Sect. II.

The temperatures which we can measure by a mercurial thermometer are confined within narrow limits. For mercury freezes at about 39° below zero, and boils at 660°. Hence we cannot employ it to measure greater heats than 660°, nor greater degrees of cold than —39°. Yet many temperatures connected with our most common processes are much higher than 660°. The heat of a common fire, the temperature at which silver, copper, and gold melts, and many other such points, offer familiar examples. Its range small.

Mr Wedgewood contrived an instrument for measuring high temperatures, which was known by the name of Wedgewood's pyrometer, and was at one time in general use. It consisted of small pieces of Cornish clay, moulded into cylinders of a determinate size, and baked in a low red heat. These pieces were of such a size as just to enter between two square brass rods, fixed on a brass plate 24 inches long, half an inch asunder at one extremity, and 0·3 inch at the other. The brass rods were divided into inches and tenths, making in all 240 divisions or degrees. When pieces of clay baked in Wedgewood's manner are exposed to heat, they shrink in their dimensions, and the shrinkage in Wedgewood's opinion was proportional to the temperature. This was the foundation of his instrument. The heat to which the clay piece was exposed was indicated when its shrinkage was measured between the brass rods. If exposed to the heat at which silver melts, it advanced between the brass rods to 22°, or 2·2 inches. If to the melting point of gold, to 32°; if to the melting point of cast iron, to 130°, and so on. Wedgewood's pyrometer.

But this pyrometer of Wedgewood has been long out of use. For it was found that if a clay piece was long exposed to a low temperature, it shrunk as much in its dimensions as if it had been exposed for a short time to a much higher temperature. In short, the time of exposure has as much effect as the tem-

Chap. I. perature upon the alteration of the dimensions of the clay pieces.*

In 1803, Guyton de Morveau presented to the French Institute a pyrometer of platinum, which measured high temperatures by the expansion of a plate of that very refractory metal.† Mr Daniell, of London, constructed a pyrometer upon similar principles, and published a description of it in the 11th volume of the Journal of the Royal Institution, in 1821. In the Philosophical Transactions for 1830,‡ Mr Daniell has given a still fuller account of his pyrometer, and of the subsequent experiments to which he had applied it; and the subject was still farther prosecuted by him in the volume of the Transactions for 1831.§

Daniell's pyrometer.

His instrument consists of a bar of platinum 10½ inches long and 0·14 inch in diameter. It is placed in a tube of black lead or earthenware, and the difference between the expansion of the platinum bar and the earthenware tube is indicated on a circular scale. This pyrometer indicates a change of about 7° of Fahrenheit's scale; or, in other words, 1° of Daniell is equal to 7° of Fahrenheit.

The following table exhibits the fusing points of several metals, determined by Mr Daniell, by means of his pyrometer.

Tin	442°
Lead	612
Zinc	773
Silver	1873
Copper	1996
Cast iron . . .	2786

Another very ingenious method of measuring high temperatures has been suggested by thermo-electricity. When two wires of two different metals are soldered together and attached to a *thermo-multiplier*,‖ if the flame of a lamp be ap-

* See, for an account of Wedgewood's pyrometer, Phil. Trans. 1782, p. 305; 1784, p. 358; 1786, p. 390.

† Ann. de Chimie, xlvi. 276. ‡ P. 257. § P. 443.

‖ An instrument consisting of a magnetic needle well poised, and encircled by a coil of wire. It will be described in the second part of this volume.

plied to the point of the junction of the wires, while another portion is kept cold, the needle is found to move, and the amount of deviation has been found proportional to the heat applied. Two platinum wires, differing in their purity, answer the purpose. Now, when two such wires having a diameter of 0·01312 inch are twisted together instead of being soldered, and heated at the junction till the temperature rises 540°, the needle is observed to deviate 8°. If we suppose that the deviation is directly as the heat applied, then, by observing the deviation of the needle we can infer the increase of temperature to which the wires have been subjected. In this way Becquerel found the rise of temperature of platinum wire, when put into various parts of a spirit lamp, to be as follows:—*

	Heat.
Just under the white flame and above the blue	2462°
In the white flame	1976
In the dark part near the wick . . .	1436

He and Brogniart tried the heat of the reverberatory furnace at Sevres, in which the porcelain is baked, and found by the same apparatus that the heat acquired by the platinum wires was as follows:—†

	Heat.
Just after being put in . .	3780°
In an hour	4188
In an hour and a half . .	4244
In $2\frac{1}{2}$ hours	4482
In 3 hours	4609

The thermometer has enlarged our notions of heat.

The thermometer, notwithstanding the unavoidable defects under which it still labours, has contributed very much to enlarge our notions respecting heat. *Heat* and *cold* constitute two of the most familiar words in our language. When either heat or cold is intense, it constitutes a sensation of the strongest kind. Both are capable of destroying life. We are accustomed to consider both heat and cold as a positive something, and it requires an attentive consideration to render it probable, that *cold* is nothing else than the absence or abstraction of heat.

* Traité de l'Electricité, iv. 3. † Ann. de Chim. et de Phys. lvii. 5.

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But the thermometer furnishes a decisive proof of the accu of this opinion, which is level to the meanest capacity. It *tracts* uninterruptedly by a gradual abstraction of heat, an this contraction continues to the very lowest point which are capable of reaching; we can form no reasonable doubt the greatest cold is produced by the abstraction of heat, as as the smallest. We have no doubt that the congelatio lead or tin takes place, when a certain quantity of hea withdrawn from these bodies. The congelation of bees' w of tallow, and of phosphorus, is produced in the very s way, and so also is the freezing of water. Yet in this case, *heat* gives place to another sensation, namely, of c We never in common life speak in this last case of a dimi tion of heat; but of an increase of cold. But the contin contraction of the thermometer, and the analogy between freezing of lead, wax, and water, soon induce the belief t both are caused in the same way; namely, by the abstract of heat. Thus our ideas of the operations of heat are gre extended. We now know that no mass of matter has ever been found totally destitute of heat. For we have no su cient evidence that pure alcohol has ever yet been froz though we are sure that it only requires a sufficient diminut of heat to produce that effect on it.

No body without heat.

Indeed, if we recollect that heat is continually emanat from surrounding bodies, and that bodies absorb it so mu the more greedily the colder they are, we cannot conceiv body altogether void of it, which is exposed in the neighbo hood of others that contain it. The coldest part of the ea of which we have any accurate knowledge, is Melville isla situated in 74° 47′ of north latitude, and 110° 48′ west lon tude from Greenwich, where Captain Parry wintered dur the year 1819-20. During the months of November, Dece ber, January, February, and March, the thermometer occasionally as low as —50°, and at some distance from ship, as —55°. In October it sank as low as —28°, in M as low as —4°. During January, it was never higher t —2°, and in February than —17°. During every montl the year it froze. The hottest month was July, and

Coldest climate of the globe.

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aximum temperature was 60°. There were only 5 months the year in which the maximum temperature exceeded 32°, ese were May, June, July, August, and September. The llowing table exhibits the maximum and minimum temperaure of the months as observed by Captain Parry:—

	Maximum.	Minimum.
January	— 2°	—47°
February	—17	—50
March	+ 6	—40
April	+32	—32
May	+47	— 4
June	+51	+28
July	+60	+32
August	+45	+22
September	+37	— 1
October	+17·5	—28
November	+ 6	—47
December	+ 6	—43

So that the highest temperature observed was 60°, and the lowest —50°, making a range of 110°. In some parts of Africa, and even of Asia, the thermometer is said to have been observed as high as 138°.

Sir John Ross, in the winter of 1832, observed the thermometer in Boothia, (N. lat. about 71°) at —60°. This is the greatest degree of natural cold hitherto observed. Captain Franklin observed the thermometer during the winter of 1820, at Fort Enterprize, near the copper-mine river, in North latitude 64° 30′, as low as —57°; and Dobell informs us that in January, 1828, the thermometer stood at —51° at Irkutsk, the capital of Eastern Siberia, in N. lat. 52° 16′ 41″, and East long. 104° 51′, and consequently not much farther north than London.

Discovery of the congelation of mercury.

Quicksilver in our climate, and indeed in every habitable climate, is usually in a liquid state; and it was long the opinion of chemists, that fluidity was a property essentially belonging to this metal. The fixing (or rendering solid) of mercury was a favourite pursuit of the alchymists, and constituted, in their opinion, one of the great steps towards the discovery of

Chap. I. the philosopher's stone. The discovery of its congelati constitutes a memorable era in chemistry; as it altered a materially improved the opinions of chemists respecting t effects of heat. It deserves, therefore, to be briefly notic here.

We are indebted to M. Braun, professor of philosophy in t Imperial Academy of St Petersburg, for the discovery of t congelation of quicksilver by cold. Dr Zeiher, professor mechanics in the same Academy, had repeated Fahrenhei experiments with freezing mixtures, before he came to set at St Petersburg; and he expected to be able to prosecute th still farther in that city, where the natural cold is frequen intense. Illness preventing him from putting his project execution, he communicated it to M. Braun, who readily und took it. A proper opportunity occurred on the 14th Dece ber, 1759, as the thermometer on that day sank in the op air to —34°. M. Braun prepared a mixture with nitrous a and pounded ice, into which a thermometer being put, su to —69°, lower by about 30° than it had fallen in any precedi experiments of this nature. Animated by the hope of prod ing still greater cold, he entered on his experiment anew, a all his pounded ice being exhausted, he was fortunately oblig to substitute snow in its place. With this fresh mixture, sank his thermometer to —100°, —244°, and at last to —35 Surprised at such results, he drew the instrument out of t mixture, and found it entire, but the quicksilver was fixed a remained immovable for above 12 minutes. On repeati the same experiment with another thermometer, graduated lower than —220°, all the mercury sank into the ball, a became solid as before; not beginning to re-ascend till afte great interval of time.

From these appearances, M. Braun concluded that the qui silver in both instruments had been frozen or solidified by t cold. He announced his opinion as a probable fact to the A demy, and prepared a new set of thermometers, in order to obt decisive evidence. On the 25th December (old style), in co pany with Æpinus, professor of physics, he repeated the expe ments, and as soon as he found the quicksilver immovable,

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broke the bulb of his thermometer. Now all doubts were removed. He obtained a solid, shining, metallic, mass, which extended under the strokes of a pestle, in hardness rather inferior to lead, and yielding a dull dead sound like that metal. Æpinus observed, at the same time, that the frozen mercury assumed a concave surface, and that congealed pieces of it sunk in fluid mercury: all evident proofs of its great contraction.

In the year 1775, Mr Hutchins repeated these experiments at Albany Fort, Hudson's Bay, congealed the mercury by cold, but was equally at a loss with Braun, to determine the true point of congelation. Mr Cavendish, to obviate his difficulties, sent him out a proper set of instruments, together with ample directions how to proceed. In the winter 1781–82, Mr Hutchins made the requisite experiments, an account of which was published by Mr Cavendish in the Philosophical Transactions for 1783. The mercury was frozen in wide glass tubes, and the thermometer to determine its freezing point was plunged into it, and kept in it during the whole process of congelation. It remained stationary during the freezing at $-38°·66$, showing clearly that this is the freezing point of that metal. Mr Cavendish showed that in the act of congealing, mercury contracts about $\frac{1}{23}$d of its bulk. This accounts for the very low point to which the thermometer appeared to sink in M. Braun's experiments. The freezing point was $-38°·66$, to which it sunk in the first place. By freezing, it diminished in bulk $\frac{1}{23}$d part, which is equivalent to 452°. So that his thermometers, supposing the true cold not to have exceeded —40°, would have indicated the low temperature of $-492°$.

Freezing point of mercury.

Having thus explained at some length the nature of the thermometer, it will be proper now to point out some of the most important new facts respecting heat, which have been acquired by means of the thermometer. These facts will serve still farther to extend our knowledge, and rectify our opinions respecting heat.

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SECTION III.—OF SPECIFIC HEAT.

It has been already observed that heat has a tenden diffuse itself from any hotter body to the colder bodies ar it. This distribution continues till no body is disposed to any more heat from the rest. When all mutual actior ceased, if we apply a thermometer to any one of the con ous bodies we shall find that they all indicate the same peratures. We must therefore adopt as one of the general laws of heat, that *all bodies communicating freely each other and exposed to no inequality of external action, ac the same temperature as indicated by the thermometer.*

Neighbouring bodies acquire the same temperature.

When bodies are brought into this state they are sa common language to be equally hot; and even chemists philosophers were long of opinion that all bodies in sucl cumstances, supposing the weight the same, contained ex the same quantity of heat. An experiment of Fahrer made at the request of Boerhaave, first led to an acc investigation of the subject.*

Experiment of Fahrenheit,

Fahrenheit took equal volumes of water and mercu different temperatures, and agitated them together. The perature produced was not the mean between that of the and mercury previous to mixture, as it would have beer equal volumes of hot and cold water been thus agitated ther. When the water, previous to agitation, was hot ar mercury cold, the new temperature resulting was greater the mean; and it was less than the mean, if the mercur hot and the water cold. To produce the mean temper it was necessary to agitate together 2 volumes of water volumes of mercury. When such measures were taken, haave assures us that the mean temperature always res whether the water or the mercury was hottest before the mi

From which the specific heats of bodies was deduced

It is very surprising that Boerhaave drew as a concl from this experiment, that heat is distributed through t in proportion to their volume, not to their weight. It w Black who first drew the proper inference from this ex ment about the year 1760, while a lecturer on chemis

* See Boerhaave's Chemistry, translated by Shaw, vol. i. p. 2

the University of Glasgow. Dr Black reasoned upon the experiment in the following manner:— Sect. III.

Let us suppose a given volume of water at 100° mixed with the same volume of quicksilver at 150°. We know that the mean temperature between 100° and 150°, is 125°, which would be produced by mixing water at 100° with an equal volume of water at 150°. But when hot quicksilver is used instead of water, the temperature of the mixture is only 120° instead of 125°. The mercury therefore has lost 30° of heat, while the water has become warmer by 20° only. And yet the heat which the water has gained is the very same which the mercury has lost. We see from this that the same quantity of heat has a greater effect in heating mercury than in heating water. If it heat mercury 3° it will heat water only 2°, or if it heat water 1° it will heat mercury 1°½. So that if we reckon the *capacity* of mercury for heat 1, the capacity of the same volume of water will be 1½. But mercury is 13½ times denser than water. Consequently to find the relative capacity of the same weights of mercury and water we have only to multiply 1·5 by 13½. The product 20¼ will give us the capacity of water for heat, if we reckon that of mercury 1; supposing Fahrenheit's experiment to have been accurately made.

The experiment of Fahrenheit was observed by Dr Black to agree very well with another experiment related by Dr Martine in his essay on the heating and cooling of bodies. He placed before a good fire and at equal distances from it, a quantity of water and an equal volume of mercury, each of them contained in equal and similar glass vessels, and each having a delicate thermometer immersed in it. He found in repeated experiments that the mercury was warmed by the fire much faster than the water; indeed almost twice as fast. After each experiment, having heated each of these two fluids to the same degree, he placed them in a cold stream of air, and found that the mercury was always cooled much faster than the water.* The reason of this result is obvious when we know that mercury has a smaller capacity for heat than water. It heats sooner than that liquid, because less heat is necessary to Of Dr Martine.

* Martine's Essays, p. 74.

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raise it the same number of degrees, than is required t
the same change in the temperature of water. It cools
than water, because it has less heat to lose than that li

Dr Black satisfied himself with establishing the fa
bodies have different capacities for heat, from these (

Of Irvine and Watt.

periments of Fahrenheit and Martine. But Dr Irvir
succeeded him as lecturer on chemistry in Glasgow, a
Watt, who was at that time a philosophical instrument
in that city, took up the subject and made many expe
to ascertain the relative capacities of different bodies
they did not think it necessary to lay the result of the

Of Dr Crawford.

ments before the public. Dr Crawford probably ins
table of *specific* heats in the first edition of his *Experim*
Heat, published in 1779.* He was therefore the
author on the subject. In the 17th volume of the Jou
Physique, published in 1780, there is a treatise on h
Magellan, containing a pretty copious table of specific
furnished, as Magellan informs us, by Mr Kirwan.

In the Memoirs of the Swedish Academy of Scien
1781, there is an ingenious Essay on the specific l

Of Wilcke.

metals, &c., by Mr J. C. Wilcke. Whether he was ac
ed with the previous labours of the British chemists
subject does not appear, as he nowhere makes the leas
sion to them: but quotes Klingenstjerna as the auth
first started the doctrine of the different capacity of bo
heat. Klingenstjerna died in 1765; and from the p
which Wilcke quotes, he appears to have reasoned fr
experiment of Fahrenheit precisely as Dr Black did.
it would seem that both Black and Klingenstjerna disc
the doctrine of the different specific heat of bodies, v
any communication with each other: but from a knowl
an experiment made long before by Fahrenheit, and
ously reasoned from by Boerhaave. Which of the tv
the precedence in point of time, I have no means of kn

Of Lavoisier and Laplace.

A set of experiments undertaken by Lavoisier and L

* I have never seen a copy of the first edition of this work; l
no doubt that it contained experiments on the capacity of bodies f
as his theory was founded on it

to determine the specific heat of various bodies, appeared in the Memoires of the Paris Academy of Sciences for 1780; not published probably till 1784. Sect. III.

After the publication of the second edition of Crawford's *Treatise on Heat*, in 1788, a considerable interval of time elapsed before any addition was made to our knowledge of the specific heats of bodies. A set of experiments to determine the specific heats of 16 different species of wood, by Professor Meyer of Erlangen, appeared in Crell's Annals for 1798. Professor Leslie of Edinburgh, in his *Treatise on Heat*, published in 1804, gave the specific heats of a few bodies determined in the same way as Professor Meyer had done that of the woods. Mr Dalton adopted the same mode of experimenting, and in the first volume of his *New System of Chemical Philosophy*, published in 1808, he gave a number of results obtained by him in that way.

Of Meyer, Leslie, and Dalton.

In 1813, a most elaborate set of experiments was published by Delaroche and Berard on the specific heats of gaseous bodies and vapours. These experiments were made in Berthollet's laboratory, and the apparatus had been constructed at the expense of that eminent chemist.* These experiments seem to have been conducted with care; but the experimenters were guilty of an oversight which prevented their results from being very near approximations to the truth. The gases on which they experimented were moist. Hence the influence of the vapour would vary, according to the specific gravity of the gas; this introduced a variable unknown quantity, which ought to have been subtracted in order to obtain the true specific heat of each gas.

Of Delaroche and Berard.

In 1823, a new set of experiments was made by Mr Haycraft,† with an apparatus resembling that of Delaroche and Berard. But he took the precaution of drying his gases with great care. This enabled him to obtain results agreeing accurately with each other, and obviously very near approximations to the truth. Indeed, his results were fully corroborated by new experiments of MM. Delarive and F. Marcet, who

Of Haycraft.

Of Delarive and Marcet.

* See Ann. de Chim. lxxxv. 72, or Annals of Philosophy, ii. 134.

† Edin. Trans. x. 195.

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determined the specific heat of no fewer than 14 gases.* Their method was to measure the time that a thermometer, plunged into each gas, took to be heated a certain number of degrees, the gas being exposed to a constant source of heat. To obviate the effect of mobility, the globe containing the gases was placed in the centre of a copper globe filled with rarefied air, and blackened within. The source of heat was a large vessel filled with water of 86°, into which the whole apparatus was plunged.

Of Avogadro.

In the year 1833, an elaborate memoir on the specific of bodies, both solid and liquid, by M. Avogadro, was published in the memoirs of the Scientific Society of Modena.† He determined the specific heat of 39 different bodies, and in a subsequent memoir, he determined the specific heats of phosphorus, arsenic, and iodine;‡ the object which he had in view was to investigate the truth of a law first noticed by Dulong and Petit, and which will be explained in a subsequent part of this section. In the year 1831, M. Neumann, professor of physics in the University of Königsberg, had published an interesting set of experiments on the specific heat of mineral bodies, having determined the specific heat of no fewer than 36 different species§—some of them the very same with those afterwards determined by Avogadro, though without any knowledge of what Neumann had already done.

Of Dulong.

Two other important memoirs on the specific heat of gaseous bodies require to be mentioned. In the month of May, 1828, M. Dulong read to the French Academy of Sciences, a set of experiments on the specific heat of elastic fluids.‖ He laid it down as a principle, that the simple gases have, under a given volume, the same specific heat. He then determined the ratio which subsists between the specific heats of gases, under a constant volume or a constant pressure. If *a* be the heat necessary to be communicated to a given weight of any

* Ann. de Chim. et de Phys. xxxv. 1.

† An extract from this paper is inserted in the Ann. de Chim. et de Phys. lv. 80.

‡ Ann. de Chim. et de Phys. lvii. 113.

§ Poggendorf's Annalen, xxiii. 1.

Ann. de Chim. et de Phys. xli. 113

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gas, in order to produce in it (maintained of a constant volume,) a given increase of temperature, and $a + b$ the heat necessary to produce the same change of temperature when the gas is permitted to expand so as to retain its primitive elasticity, then $\frac{a+b}{a} = 1 + \frac{b}{a}$ expresses the ratio in question. At this quantity Dulong arrived in the following manner:—

The Newtonian formula for the velocity of sound, $v = \sqrt{\frac{gh}{d} \times (1 \cdot 00375t)}$ has been long known to give results less than the truth. Laplace first showed that in order to give a true result, this expression must be multiplied by the square root of the relation, between the specific heat of air under a constant volume, and a constant pressure. If then the velocity of sound in atmospheric air be determined experimentally, and this velocity be divided by Newton's expression, the quotient will be $\sqrt{\frac{a+b}{a}}$. And this method may be extended to all the gases, provided we can determine the velocity of sound in each. This velocity Dulong deduced from experiments on a flute-like pipe blown through by the different gases. From the note struck by each, he was enabled to calculate the length of a single vibration, and the number performed in a given time by each elastic fluid tried. The velocities thus obtained, were divided by their values as given by Newton's formula. The squares of the quotients represented $1 + \frac{b}{a}$, or the relation for each gas between its specific heat, under a constant volume and a constant pressure. In this way he determined the specific heats of 7 different gases, both under a constant volume and a constant pressure.

Of Dr Apjohn.

In the year 1836, Dr Apjohn, of Dublin, read a paper before the Royal Irish Academy, the object of which was to show that the formula $f' = f'' - \frac{48ad}{e} \times \frac{p}{30}$ expresses the relation between the indications of the wet bulb hygrometer and the dew point. e is the heat of elasticity of vapour at the tem-

Chap. I. perature t' of the hygrometer; a the specific heat of air: d $t-t'$ the difference between the temperature of air and h grometer; and f', f'' the elastic forces of the vapour water, at the temperatures of the hygrometer and dew point

In 1837, Dr Apjohn read another paper before the Dubl Academy, to show that from the preceding formula, tl specific heat of the gases may be deduced. For, if the de point and temperature of the hygrometer be known, we ha a (the specific heat under a given volume of the gas,) $\left(f'-f''\right)\frac{e}{48d}\times\frac{p}{30}$. This equation he modifies so as reduce it to $a=\frac{ef'}{48d}\times\frac{p}{30}$. This expression involves no u known quantity but d, which may be found by determining tl stationary temperature to which, when in a state of perfe desiccation, it brings the wet bulb thermometer. He made set of experiments to determine this point by passing t gases respectively over a wet bulb and dry thermometer e closed in a glass tube, and noting the point in each when t wet bulb thermometer became stationary; and in this w determined the specific heats of 7 different gases.

Such are the principal sources from which our knowled of the specific heat of bodies is derived. It has been asc tained that every body requires a certain quantity of heat increase its temperature a certain number of degrees: a scarcely any two bodies agree with each other in the quanti requisite. The heat thus necessary is called the *specific he* of the body. Dr Black and his followers, Dr Irvine and M Watt, distinguished this property of bodies by the phrase *cap city of bodies for heat*. But the phrase *specific heat* has prevaile being considered as a simple specification of the matter of fac

Few bodies have a higher specific heat than *water*. Part on this account, but chiefly on account of the facility wi which it may be everywhere procured, it has been made choi of as a standard to which the specific heat of other bodies referred. Its specific heat is considered as (1.) unity. If substance has twice the specific heat of water, we say that i specific heat is 2; if thrice, that it is 3, and so on. If it h

half or one-third the specific gravity of water, we say that its specific heat is 0·5, or 0·33, and so of the others. Sect. III.

Three different modes of determining the specific heats of bodies have been contrived.

1. The first and oldest method is to mix determinate weights of the body whose specific heat we want, and of water at different temperatures together, to observe the new temperature produced by the mixture, and from this to deduce the specific heat of the body under trial. For example, if we take 1 pound of water of the temperature 100°, and 10 pounds of mercury of the temperature 200°, and agitate them together in a glass phial, after the agitation, we shall find that both liquids will have acquired a uniform temperature. The water will have become hotter, and the mercury colder. Abstracting for the present the effect of the phial, this new temperature will be nearly 125°; so that the water will have gained 25° of heat, while the mercury will have lost 75°. Thus the mercury has lost three times as many degrees of temperature as the water has gained. It is obvious, therefore, that the same quantity of heat produces three times as much effect in raising the temperature of 10 lbs. of mercury, as in raising the temperature of 1 lb. of water. Therefore, if we take equal weights of these liquids, it is clear that the same quantity of heat will produce thirty times the effect on mercury as on water. Therefore, the specific heat of water is thirty times as great as that of mercury. If the specific heat of water be 1, that of mercury must be 0·033. Specific heat determined by mixture.

In making such experiments, it is always necessary to attend to the heat communicated or abstracted by the vessel in which the experiment is made. Suppose, for example, that the pound of water of 100° was standing in a glass phial weighing half a pound, and that we pour into it the ten pounds of mercury of the temperature 200°, it is clear that the surplus heat in the mercury would not be all expended in raising the temperature of the water; a portion of it would go to heat the phial, which would acquire as great an addition of temperature or nearly so as the water. If the specific heat of the phial were as great as that of the water, it is plain that one-

Chap. I. third of the heat would go into the phial; so that the temperature after mixture, would only be $116°\frac{2}{3}$, instead of 125°. But the specific heat of glass is only about $\frac{1}{6}$th of that of water. So that only $\frac{1}{6}$ of $\frac{1}{3}$ or $\frac{1}{18}$th of the surplus heat would be employed in heating the phial. The temperature after mixture instead of 125° would be only $123°\frac{2}{3}$.

The easiest way of avoiding this error is to make two experiments. The first by pouring the hot mercury into the cold water, the second by pouring the cold water into the hot mercury; taking care that both trials are made in the same vessel. By taking the mean of the two experiments, we obtain very nearly the true temperature of the mixture.

It was in this way that the experiments of Irvine, Kirwan, Crawford, Wilcke, and Avogadro were conducted. Dr Irvine reduced the whole to the following mathematical formula.

Let the weight of the water be W, its temp. w.
———————— body be B, ——— b.
Let the temperature after mixture be m.

$$\text{The specific heat of } B = \frac{W\ (m-w)}{B\ (b-m)}.$$

In this formula the water is supposed to be coldest. If the water be hot and the body be cold, which in some cases may be the more convenient mode of making the experiment, the

$$\text{Specific heat of } B = \frac{W\ (w-m)}{B\ (m-b)}.$$

This mode of experimenting, though theoretically easy, is in reality so difficult, that the results obtained by it are probably not very near approximations to the truth. The mode of allowing for the heat lost during the experiment, which has been uniformly employed, is certainly inaccurate.

By the calorimeter. 2. The second method of determining the specific heats of bodies was contrived by Lavoisier and Laplace, and employed by them in determining the specific heats of about eight different substances. This method appears abundantly simple; but it has failed in the hands of every one who has attempted it since the publications of their experiments. An instrument was contrived, to which Lavoisier gave the name of *calorimeter*. It consists of three circular vessels nearly inscribed into each

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other, so as to form three different apartments, one within the other. These three we shall call the *interior*, *middle*, and *external cavities*. The interior cavity into which the substances submitted to experiment are put, is composed of a grating or cage of iron wire, supported by several iron bars. Its opening or mouth is covered by a lid, which is composed of the same materials. The middle cavity is filled with ice. This ice is supported by a grate, and under the grate is placed a sieve. The external cavity is also filled with ice. We have remarked already, that no caloric can pass through ice at 32°. It can enter ice, indeed, but it remains in it, and is employed in melting it. The quantity of ice melted, then, is a measure of the heat which has entered into the ice. The exterior and middle cavities being filled with ice, all the water is allowed to drain away, and the temperature of the interior cavity to come down to 32°. Then the substance, the specific heat of which is to be ascertained, is heated a certain number of degrees, suppose to 212°, and immediately put into the interior cavity enclosed in a thin vessel. As it cools, it melts the ice in the middle cavity. In proportion as it melts, the water runs through the grate and sieve, and falls through the conical funnel and the tube into a vessel placed below to receive it. The external cavity is filled with ice, in order to prevent the external air from approaching the ice in the middle cavity, and melting part of it. The water produced from it is carried off through a pipe. The external air ought never to be below 32°, nor above 41°. In the first case, the ice in the middle cavity might be cooled too low; in the last, a current of air passes through the machine, and carries off some of the caloric. By putting various substances at the same temperature into this machine, and observing how much ice each of them melted in cooling down to 32°, it was easy to ascertain the specific heat of each. Thus if water, in cooling from 212° to 32°, melted one pound of ice, and spermaceti oil 0·5 of a pound; the specific heat of water was 1, and that of the oil 0·5.

By the time of cooling.

3. The third mode of determining the specific heats of bodies was suggested by Dr Black many years ago; but seems to have been first employed by Professor Meyer of Erlangen in 1797,

Chap. I. to determine the specific heat of woods. He took spherical pieces of wood of the same size, and dried to the same degree. Each of these was heated to the same point and suspended in a cold room. The time of cooling a certain number of degrees was noted, and the specific heats of the same bulk of woods were considered as proportional to these times; and when the times were divided by the specific gravities, the quotients represented the specific heats of equal weights. Professor Leslie extended this method to liquids as well as solids. It is not only easier than either of the other two; but likewise susceptible of much greater exactness.

When we take the specific heat of solids in this way, nothing more is necessary than to have each body of the same size, to measure the time that each takes to lose a certain number of degrees of temperature, and to divide this time by the specific gravity of each body. The quotient will give the specific heat of equal weights of each body. But when we take the specific heat of liquids in this way, it is obvious that they must be placed in a vessel of some kind. It will be requisite, by a previous set of experiments, to ascertain the time that this vessel takes to cool the requisite number of degrees. Because this quantity must be subtracted from every result, in order to obtain the true time that the liquid took to cool. The best way is to employ the same glass vessel in every experiment. Let us suppose a glass vessel to take 5 minutes to cool from 130° to 80°. Suppose it filled with water of 130°, and when so filled to take 40 minutes to sink to 80°. When filled with sulphuric acid of 130°, let it take 25·8 minutes to sink from 130° to 80°. It is obvious that in order to get the true times which these liquids took to cool 50°, we must subtract from each result the 5 minutes occupied by the hot glass in losing 30° of heat. So that equal bulks of water and sulphuric acid lose 50°, the first in 35′, the second in 20′·8. The specific gravity of sulphuric acid being 1·847 we have $\frac{20\cdot 8}{1\cdot 847} = 11\cdot 26$. So that if the specific heat of water be 35, that of sulphuric acid will be 11·26. And 35 : 11·26 : : 1 : 0·32 = specific heat of sulphuric acid, if the specific heat of water be 1.

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Neumann in his experiments sometimes employed the method mixture and sometimes that of cooling. In general, both hods gave very nearly the same result.

Before attempting to give a theory of specific heat, I shall the following table containing all the specific heats hither-determined (so far as I am acquainted with the subject) be-the reader.

Table of specific heats.

I. SIMPLE BODIES.

rcoal	0·257†† 0·2631*
sphorus	0·385††
hur	0·183† 0·188(c) 0·19D 0·209N
no	0·089††
ury	0·0290‡ 0·0330(c) 0·033† 0·0357* 0·0496D
nic	0·081††
ium	0·0314(c)
nuth	0·027N 0·0288(c) 0·04D 0·043§
r	0·0557(c) 0·08D 0·082§
e	0·0927(c) 0·0943* 0·10D 0·102§
lurium	0·0912(c)
kel	0·10D 0·1035(c)
balt	0·1498(c)
n	0·1099‡ 0·1100(c) 0·125† 0·126§ 0·1269* 0·13D 0·143(a)
Copper	0·0949(c) 0·11D 0·1123*
Lead	0·0293(c) 0·0352* 0·04D 0·042§ 0·050†
Gold	0·0298(c) 0·050† 0·05D
Antimony	0·047N 0·06D 0·063§ 0·086†
Tin	0·0514(c) 0·060§ 0·068† 0·07D 0·0704*

II. ALLOYS.

Gun metal	0·11‖
Brass (8·356)	0·11D 0·1123* 0·116§

III. CHLORIDES.

Calomel	0·041††
Corrosive sublimate	0·069††
Common salt	0·221†† 0·23D 0·226G
Chloride of potassium	0·184††
Chloride of calcium	0·194††

IV. SALTS.

Nitre	0·269††

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Nitrate of soda	0·240††
Anhydrous sulphate of lime	0·190††
—— of potash	0·169††
—— of soda	0·263††
—— of iron	0·145††
—— of copper	0·180††
—— of zinc	0·213††
Anhydrous carbonate of potash	0·237††
—— of soda	0·306††
Hydrous sulphate of of lime	0·302††
Sulphate of barytes	0·1060N 0·1071N 0·1072N
Sulphate of strontian	0·130N
Anhydrite .	0·1854N
Arragonite .	0·1966N
Calcerous spar .	0·2015N
Chalk . .	0·2564* 0·27D
Bitter spar .	0·213N 0·2179N

V. OXIDES.

Alumina .	0·200††
Silica . .	0·179††
Quicklime .	0·205N 0·179†† 0·2168‡ 0·30D
Magnesia .	0·276N
Oxide of iron .	0·320†
Peroxide of iron	0·1666*
Antimonious acid	0·130N
Suboxide of copper	0·1073N
Oxide of copper	0·137N 0·146††
Protoxide of lead	0·05†† 0·068* 0·068†
Red lead . .	0·061W 0·072††
Red oxide of mercury	0·049N 0·050††
Oxide of zinc	0·132N 0·1369* 0·141††
Protoxide of tin	0·096††
Peroxide of tin	0·111††
Tinstone .	0·0895N
Oxide of chromium	0·196N
Oxide of uranium	0·106N
Peroxide of manganese	0·191††
Titanic acid .	0·1724N

VI. HYDRATED OXIDES.

Hydrate of potash	0·358††
—— of lime	0·40D
—— of alumina	0·420††
Hydrated peroxide of iron	0·186††

VII. SULPHURETS.

Sulphuret of iron	0·135††
Persulphuret of iron	0·1323N
Galena .	0·044N 0·046†† 0·053N
Cinnabar .	0·048†† 0·052N
Orpiment .	0·105††
Realgar .	0·130N
Blende . .	0·113N
Molybdena .	0·102N

VIII. MINERALS.

Gurhofian .	0·2168N
Magnesite .	0·2270N
Sparry iron ore	0·1820N
Calamine .	0·1712N
Red iron ore .	0·166N
Iron glance .	0·163N
Pitch ore .	0·106N

IX. GLASS, &c.

Glass . .	0·1770(
Flint glass .	0·19D 0·174†
Swedish glass (2·386)	0·187
Crystal . .	0·189 0·1929
Crown glass .	0·200
Stoneware .	0·195
Agate . .	0·195

X. ANIMAL SOLIDS.

hide with hair	0·7870*
gs of a sheep	0·7690*
n of ox beef	0·7400*

XI. VEGETABLE SOLIDS.

s sylvestris	0·65¶
s abies .	0·60¶
Europæa .	0·62¶
s picea .	0·58¶
s malus .	0·57¶
la alnus .	0·53¶
on . .	0·53
rcus robur / silis	0·51¶
inus excelsior	0·51¶
s communis	0·50¶
. .	0·5060*
e beans .	0·5020*
of the pine-tree	0·5000*
. .	0·4920*
s Sylvatica	0·49¶
pinus betulus	0·48¶
la alba .	0·48¶
at . .	0·4770*
. .	0·47¶
rcus robur pe- / nculata	0·45¶
nus domestica	0·44¶
spyrus ebenum	9·43¶
rley . .	0·4210*
s . .	0·4160*
coal . .	0·28D
	0·2777*
rcoal . .	0·2631*
nders . .	0·1923*

XII. LIQUIDS.

Water at 32° .		1·000
—— at 212° .		1·0176N
Atoms Nitric acid.	Atoms Water.	
1	+ 1	0·4416T
1	+ 2	0·5138
1	+ 3	0·5553
1	+ 4	0·5834
1	+ 5	0·6021
1	+ 6	0·6418
1	+ 7	0·6495
1	+ 8	0·6832
1	+ 9	0·6941
1	+10	0·7239
Atoms Sulphuric acid.	Atoms Water.	
1	+ 1	0·3593T
1	+ 2	0·4707
1	+ 3	0·4786
1	+ 4	0·5228
1	+ 5	0·5690
1	+ 6	0·6091
1	+ 7	0·6429
1	+ 8	0·6699
1	+ 9	0·7003
1	+10	0·7201
Alcohol (absolute)		0·6600T
Atoms Alcohol.	Atoms Water.	
4	+ 1	0·6775
3	+ 1	0·7576
2	+ 1	0·8034
1	+ 1	0·8466
1	+ 2	0·9210
1	+ 3	0·9915
1	+ 4	0·9962(*)

(*) Specific gravity of nitric acid.

Atoms acid.	Atoms water.	
1	+ 1·37	1·504
1	+ 2	1·4862
1	+ 3	1·4477
1	+ 4	1·4177
1	+ 5	1·4005
1	+ 6	1·3724
1	+ 7	1·3598
1	+ 8	1·3235
1	+ 9	1·3007
1	+10	1·2815

Specific gravity of sulphuric acid.

Atoms acid.	Atoms water.	
1	+ 1	1·8422
1	+ 2	1·7837
1	+ 3	1·6588
1	+ 4	1·5593
1	+ 5	1·4737
1	+ 6	1·4170
1	+ 7	1·3730
1	+ 8	1·3417
1	+ 9	1·3105
1	+10	1·2845

Specific gravity of alcohol.

Absolute alcohol		0·795
Atoms alcohol.	Atoms water.	
4	+ 1	0·8179
3	+ 1	0·8259
2	+ 1	0·8384
1	+ 1	0·8672
1	+ 2	0·9042
1	+ 3	0·9266
1	+ 4	0·9412

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Mur. acid (1·122)	0·68†	Spermaceti oil	0·500*
(1·153)	0·60D		0·52D
Acetic acid (1·056)	0·66D	Whale oil .	0·530T
Vinegar . .	0·92D	Oil of turpentine	0·3385¢
Solution of potash (1·346) .	0·759†		0·400(a
Solution of ammonia (0·997) .	0·708†		0·462(d
(0·948) .	1·03D		0·472†
Ether (0·729) .	0·520(d)	Naphtha . .	0·4151§
	0·54329‖	Liquid spermaceti	0·320(a
(0·76) .	0·66D	Arterial blood .	1·030*
Olive oil . .	0·43849‖		0·913(b
	0·50L	Venous blood .	0·8928*
	0·718†		0·903(b
Linseed oil .	0·45192‖	Cow's milk .	0·9999*
	0·528†		0·98D

XIII. GASES.

The determination of the specific heat of gases is so diffi that no experiments hitherto published have obtained gen confidence. It will be proper, on that account, to state se rately the principal results obtained.

(1.) *Experiments of Delaroche and Berard.*

1. *Gases referred to air.*

	Same bulk.	Same weight.
Air . . .	1·0000 . .	1·0000
Hydrogen . .	0·9033 . .	12·3401
Carbonic acid .	1·2583 . .	0·8280
Oxygen . .	0·9765 . .	0·8848
Azote . . .	1·0000 . .	1·0318
Protoxide of azote .	1·3503 . .	0·8878

* Crawford; † Kirwan; ‡ Lavoisier and Laplace; § Wilcke; ¶ M L. Leslie; ‖ Count Rumford; D. Dalton, New System of Chemical P sophy, p. 62. (a) Irvine, Essays, p. 84 and 88. (b) John Davy, Trans. 1814, p. 593. (c) Dulong and Petit, Annals of Philosophy, 164, and xiv. 189. (d) Despretz, Ann. de Chim. et de Phys. xxiv. †† Avogadro, Ann. de Chim et de Phys. lv. 92. N. Neumann, Poggend Annalen, xxiii. 30. G. Gadolm, Kong. Vet. Acad. Handl., 1784, p. T. Thomson.

	Same bulk.	Same weight.
Olefiant gas	1·5530	1·5768
Carbonic oxide	1·0340	1·0805

2. *Gases referred to water.*

	Same weight.
Water	1·0000
Air	0·2669
Hydrogen	3·2936
Carbonic acid	0·2210
Oxygen	0·2361
Azote	0·2754
Protoxide of azote	0·2369
Olefiant gas	0·4207
Carbonic acid	0·2884
Aqueous vapour	0·8470

(2.) *Determinations of Haycraft, Marcet, and Delaroche.*

According to these experimenters, the same volumes of all gases under the same pressure, have the same specific heat; consequently the specific heats (reckoned by weight) are inversely as the specific gravities; therefore, if we reckon the specific heat of oxygen 1, which is most convenient, the specific heats of the gases are as follows:—

Gases.	Specific heat.
Oxygen gas	1
Hydrogen	16
Carbon vapour	2·66
Carburetted hydrogen	2·00
Ammonia	1·8824
Steam	1·7778
Phosphorus vapour	1·3333
Phosphuretted hydrogen	1·2307
Hydrocyanic acid vapour	1·1852
Bihydroguretted phosphorus	1·1429
Azotic gas	1·2429
Olefiant gas	1·1429
Carbonic oxide	1·1429
Deutoxide of azote	1·0666
Sulphur vapour	1·

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Gases.	Specific heat.
Sulphuretted hydrogen	0·94118
Muriatic acid	0·86486
Carbonic acid	0·72727
Protoxide of azote	0·72727
Cyanogen	0·61538
Tellurium vapour	0·5
Sulphurous acid	0·5
Telluretted hydrogen	0·48485
Fluoboric acid	0·47059
Chlorine	0·44444
Arsenic vapour	0·42105
Sulphuret of carbon vapour	0·42105
Arseniuretted hydrogen	0·41026
Sulphuric acid vapour	0·4
Selenium vapour	0·4
Seleniétted hydrogen	0·39024
Chlorocarbonic acid	0·32
Fluosilicic acid	0·30769
Hydriodic acid	0·256
Iodine vapour	0·12903

The experiments of Marcet and Delarive were made unde a pressure of 25·59 inches of mercury. It appears from thei trials, that as the gas is dilated the specific heat undergoes a slow diminution; as will appear from the following experiment which they made on common air.*

Pressure in inches of mercury.		Centesimal.	
25·59	the volume of air heated	6°·33	in 5·
23·22	—	6·55	—
19·17	—	6·90	
14·76	—	7·01	—
10·16	—	7·30	—

This is precisely what might have been expected; though th rate of diminution appears much slower than could have bee anticipated. This part of the subject still requires farthe elucidation.

* Ann. de Chim. et de Phys. xxxv. 28.

(3.) *Specific heats of the gases, according to M. Dulong.*

	Volume constant.	Pressure constant.
Air	1·000	1·000
Oxygen	1·000	1·000
Hydrogen	1·000	1·000
Carbonic acid	1·249	1·175
Carbonic oxide	1·000	1·000
Protoxide of azote	1·227	1·160
Olefiant gas	1·754	1·531

(4.) *Specific heats of equal volumes of gases, as determined by Dr Apjohn.*

	Water = 1·000	Air = 1·000
Air	0·2670	1·000
Azote	0·2799	1·048
Oxygen	0·2154	0·808
Hydrogen	0·3896	1·459
Carbonic acid	0·3192	1·195
Carbonic oxide	0·2660	0·996
Protoxide of azote	0·3186	1·193

Having stated the facts which have been determined experimentally, let us now endeavour to establish a theory of latent heat.

It was observed by Dulong and Petit, that if the atomic weight of a body be multiplied by its specific heat, the product is a constant quantity. The following table will enable us to judge of the truth of this observation:—

	Atomic Weight.	Sp. Heat.	Product.	
Carbon	1·50	0·257	0·385	
Sulphur	2·00	0·188	0·376	
Arsenic	4·75	0·081	0·385	
Antimony	8·00	0·047	0·376	
Tellurium	4·00	0·0912	0·364	
Iron	3·50	0·11	0·385	
Nickel	3·25	0·1035	0·336	
Zinc	4·125	0·0927	0·383	Mean 0·374
Lead	13·00	0·029	0·377	
Tin	7·25	0·051	0·370	
Copper	4	0·095	0·376	
Bismuth	9	0·04	0·360	
Mercury	12·5	0·029	0·3625	
Gold	12·5	0·0298	0·373	
Platinum	12	0·031	0·372	
Iodine	15·75	0·089	0·40175	

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The products in the third column, obtained by multi together the first and second columns, do not absolutely in consequence doubtless of errors in the determinat the specific heats of the different bodies. But the a mation is sufficiently evident, and the mean of the wh teen bodies contained in the table gives 0·374 for the c quantity obtained by multiplying the atomic weight specific heat.

It seems sufficiently established by this table, th atomic weight of a simple substance multiplied by its heat is a constant quantity. And this quantity appear 0·374 or 0·375. The conclusion to be drawn from obviously, that the *same quantity of heat is attached atom*. The reason why the specific heats appear differ that the weight of the atoms of the different bodies diffe each other. But the specific heat of each individua is absolutely the same.

Let us now examine the product obtained, when the weight of binary compounds is multiplied into their heat.

	Atomic Weight.	Sp. Heat.	Product.	
Alumina	2·25	0·200	0·450	
Silica	2	0·179	0·358	
Lime	3·5	0·205	0·717	
Magnesia	2·5	0·276	0·690	
Black oxide of copper	5	0·146	0·730	
Protoxide of lead	14	0·05	0·700	
Oxide of zinc	5·125	0·141	0·722	Mean
Protoxide of tin	8·25	0·096	0·775	
Oxide of chromium	5	0·196	0·980	
Oxide of uranium	27	0·106	0·786	

If we leave out the first two of these substances, th quantity obtained by multiplying the atomic weight eight remaining substances into the specific heat, is Now, 0·375 × 2 = 0·750, a number approaching very to 0·762. From this it would seem that each of the at a binary compound, retains all the heat with which united previously to its entering into the state of a com

Probably the atomic weight of silica and alumina i

as much as we reckon it. If this be the case, the product of the atomic weight into the specific heat will be

Alumina	. .	0·900	Mean 0·808.
Silica	. .	0·716	

this mean approaches 0·750.

It would appear from this, that the specific heat of binary compounds multiplied into the atomic weight, gives the constant product 0·75. But this law is not so general as the preceding; for the chlorides seem to contain twice as much heat as the oxides; or the product of the atomic weight into the specific heat is 1·5, instead of 0·75. This will appear from the following table:—

	Atomic Weight.	Sp. Heat.	Product.	
Corrosive sublimate	17	0·069	1·173	Mean 1·484.
Common salt .	7·5	0·221	1·657	
Chloride of potassium	9·5	6·184	1·748	
Chloride of calcium	7	0·194	1·358	

But the sulphurets come under the general law, as will appear from the following table:—

	Atomic Weight.	Sp. Heat.	Product.	
Sulphuret of iron	5·5	0·135	0·742	Mean 0·698.
Galena . .	15	0·044	0·660	
Cinnabar . .	14·5	0·048	0·696	
Blende . .	6·125	0·113	0·692	

This mean approaches sufficiently near 0·75, to show that each atom of a sulphuret retains all its heat.

Water is doubtless a ternary compound, of two atoms of hydrogen, and one atom of oxygen; its atomic weight being 1·125, and its specific heat 1, the product is 1·125. This is $0·375 \times 3 = 1·125$. Hence it appears that in water each atom retains all the heat which surrounded it or adhered to it before combination.

We may conclude, I conceive, from what has been stated, that the specific heat of a simple body is always obtained by dividing the constant quantity 0·375 by the atomic weight. Let the atomic weight of a simple body be a, and its specific heat x, and let $c = 0·375$, then $x = \frac{c}{a}$.

Chap. I. The following table, calculated from this law, exhibits specific heat of all the simple bodies at present known :—

	Atomic weight.	Specific heat.
Oxygen	1	0·375
Chlorine	4·5	0·083
Bromine	10	0·0375
Iodine	15·75	0·0238
Hydrogen	0·0625	6
Azote	1·75	0·214
Carbon	1·5	0·25
Boron	1	0·375
Silicon	2	0·1875
Phosphorus	1 ?	0·375
Sulphur	2	0·1875
Selenium	5	0·075
Tellurium	4	0·0937
Arsenic	4·75	0·079
Antimony	8	0·047
Chromium	4	0·0937
Uranium	26	0·0148
Vanadium	8·5	0·0441
Molybdenum	6	0·0625
Tungsten	12·5	0·03
Columbium	22·75	0·0164
Titanium	3·25	0·115
Potassium	5	0·075
Sodium	3	0·125
Lithium	0·75	0·5
Barium	8·5	0·044
Strontium	5·5	0·068
Calcium	2·5	0·15
Magnesium	1·5	0·25
Aluminum	2·25	0·1666
Glucinum	2·25	0·1666
Yttrium	4·5	0·0833
Cerium	5·5	0·05
Zirconium	2·75	0·136
Thorium	7·5	0·05
Iron	3·5	0·1071
Manganese	3·5	0·1071

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	Atomic weight.	Specific heat.
Nickel	3·25	0·1153
Cobalt	3·25	0·1153
Zinc	4·125	0·0909
Cadmium	7	0·05357
Lead	13	0·0288
Tin	7·25	0·0517
Copper	4	0·0937
Bismuth	9	0·04166
Mercury	12·5	0·03
Silver	13·5	0·0277
Gold	12·5	0·03
Platinum	12	0·03125
Palladium	6·75	0·0555
Rhodium	6·75	0·0555
Iridium	12·25	0·03061
Osmium	12·5	0·03
Protoxide of iron	4·5	0·1666
Protoxide of manganese	4·5	0·1666
Protoxide of nickel	4·25	0·1764
Protoxide of cobalt	4·25	0·1764
Protoxide of zinc	5·125	0·1463
Protoxide of cadmium	8	0·0945
Protoxide of lead	14	0·05357
Protoxide of tin	8·25	0·0909
Protoxide of copper	5	0·15
Yellow oxide of bismuth	10	0·075
Red oxide of mercury	13·5	0·05555
Black oxide of silver	14·5	0·0517
Sulphuret of iron	5·5	0·068
Galena	15	0·05
Cinnabar	14·5	0·0517
Blende	6·125	0·124

A comparison of this table with the results of the different experimenters will enable the reader to judge how nearly they have approximated to the truth.

It is exceedingly probable that the specific heat of binary compounds will be obtained by dividing $0·75=0·375\times 2$ by the atomic weight of each body. The following table exhibits a specimen of specific heats determined in this way.

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	Atomic weights.	Specific heats.
Potash	6	0·125
Soda	4	0·1875
Lithia	1·75	0·4285
Barytes	9·5	0·0789
Strontian	6·5	0·1153
Lime	3·5	0·2143
Magnesia	2·5	0·3

But the chlorides, unless (as is probable) we are mistaken about their constitution, do not come under the law of binary compounds, but of quaternary compounds: their specific heat being obtained by dividing $1·5 = 0·375 \times 2$ by their atomic weights, as may be seen by the following table:—

	Atomic weights.	Specific heats.
Corrosive sublimate	17	0·0882
Common salt	7·5	0·2
Chloride of potassium	9·5	0·1579
Chloride of calcium	7	0·2143

The specific heat of ternary compounds is obtained by dividing $1·125 = 0·375 \times 3$ by the atomic weight of the body. Thus the specific heat of water is unity; for its atomic weight is 1·125 and $\frac{1·125}{1·125} = 1$.

It would be hazardous, in the present state of our knowledge, to carry the calculation of specific heats into more complicated compounds. I have little doubt that it holds at least as far as quaternary and quinternary compounds; and hereafter, the knowledge of the specific heat of such bodies will be a most important means of determining not only their atomic weight, but also the number of atoms which enter into their compositions. But there are some circumstances connected with the specific heat of bodies that deserve to be particularly noticed in this place.

1. It is generally admitted by chemists that the specific heat of bodies increases with their temperature. Thus, according to Neumann,* the specific heat of water at 81° and 212° is as follows:—

at 81°	1
212°	1·0176

* Poggendorff's Annalen, xxiii. 53.

an augmentation approaching to 2 per cent. If the theory of specific heat, which I have attempted to establish in the preceding pages, be correct, it is obvious that the augmentations of heat necessary to produce a given increase of temperature, as the heat of the body increases, must be distinguished from the specific heat of the body, which must be constant at all temperatures. I conceive it to be produced in this way. Heat has the property of augmenting the bulk of bodies, and, of course, of causing the atoms of which they are composed to recede farther from each other. This increase of bulk cannot be ascribed to any thing else than a repulsion between the particles of heat which surrounds the respective atoms. This repulsion increases with the temperature, because the quantity of heat surrounding each atom increases. But as the distance between the centres of the respective atoms increases, the repulsion of the heat must proportionally diminish, and of course a greater quantity of heat surrounding each atom becomes requisite to maintain the repulsion which determines the distance between the atoms.

I shall here state the experiments of Dulong and Petit to show the increase of the specific heat of bodies as their temperature augments. Their method was to heat the body under examination to different temperatures, and then to plunge it into cold water. The change of temperature in the water enabled them to calculate the heat which the hot body had lost, and thence to determine its specific heat. They have not given us the details of their experiments; but they assure us that they were made with great care, and that the results are very near the truth. They found that the specific heat of every substance is greater at high temperatures than at low temperatures, and that it always increases with the temperatures. Thus the specific heat of iron, at different temperatures, they found as follows: —

Centigrade.	Fahrenheit.	Sp. heat.
From 0° to 100°	32° to 212°	0·1098
0 to 200	32 to 392	0·1150
0 to 300	32 to 572	0·1218
0 to 350	32 to 662	0·1255

Chap. I.

The following table exhibits the specific heats at different temperatures of the other bodies tried by them:—

	Sp. heats between 0° and 100° cent.	Sp. heats between 0° and 300° cent.
Mercury	0·0330	0·0350
Zinc	0·0927	0·1015
Antimony	0·0507	0·0549
Silver	0·0557	0·0611
Copper	0·0949	0·1013
Platinum	0·0355	0·0355
Glass	0·1770	0·1900*

2. It has been long known that when liquids upon being mixed enter into chemical combination with each other, the temperature changes, and in general, heat is evolved. Thus, when we agitate together mixtures of water and alcohol, water and nitric acid, water and sulphuric acid, the respective liquids combine, and do not again separate though left at rest. Now, at the instant when these liquids unite with each other, there is a considerable evolution of heat.

Dr Irvine was the first person who attempted to account for this increase of temperature, and his explanation has been generally admitted as satisfactory; though I am not aware of any other person except Gadolin, who has attempted to put it to the test of experiment. When two liquids evolve heat in the instant of combination, the reason according to Dr Irvine is, that the specific heat of the compound is less than the mean of the specific heat of its two constituents; hence it requires less heat than it did before the combination to maintain the temperature. This surplus heat escapes and occasions the augmentation of temperature. Irvine employed this explanation as a means of arriving at a knowledge of the real zero. He found that when equal bulks of sulphuric acid and water are mixed together, the thermometer rises from 57° to 212°; or the heat evolved amounts to 155°.

He stated the specific heat of sulphuric acid to be 0·333, or the third part of that of water; while the specific heat of a mixture of equal volumes of acid and water was 0·52. But the

* Annals of Philosophy, xiii. 167.

mean specific heat of equal volumes of acid and water is 0·57. The difference between these two numbers is 0·05, or about $\frac{1}{10}$th of the whole heat. Now, the heat evolved, he conceived to be the consequence of this diminution. It amounted to 155°, and was about $\frac{1}{10}$th of the whole heat contained in the liquids at 57°. Consequently, the real zero is 1550° below 57°, or about 1500° below the zero of Fahrenheit. Sect. III.

Before we admit the validity of this very ingenious and plausible theory, we must be sure of the accuracy of the facts. The only experiments on the subject with which I am acquainted, were made by Professor Gadolin of Abo.* His experiments were made upon solutions of common salt, and upon mixtures of sulphuric acid and water; but as he gives nowhere the specific gravities of the substances used, and as the atomic proportions were not attended to, we have not sufficient data to warrant the conclusions which Gadolin has drawn.

I made a series of experiments upon mixtures of water with alcohol, nitric acid, and sulphuric acid; and I shall here state the result of these experiments.

(1.) *Alcohol.*

The following table exhibits the specific gravity of absolute alcohol, and of mixtures of alcohol and water, in atomic proportions at the temperature of 60°:—

			Real specific gravity.	Mean specific gravity.	Differences.
Absolute alcohol			0·795		
Alcohol.		Water.			
4 atoms	+	1 atom	0·8179	0·8094	+0·00617
3	+	1	0·8259	0·8138	+0·01206
2	+	1	0·8384	0·8222	+0·01619
1	+	1	0·8672	0·8433	+0·02392
1	+	2	0·9042	0·8733	+0·03084
1	+	3	0·9266	0·8937	+0·03289
1	+	4	0·9412	0·9084	+0·03262
1	+	5	0·9509	0·9196	+0·03130
1	+	6	0·9576	0·9283	+0·02930
1	+	7	0·9624	0·9353	+0·02708
1	+	8	0·9659	0·9411	+0·02486
1	+	9	0·9687	0·9459	+0·02278
1	+	10	0·9709	0·9500	+0·02090

* Kong. Vet. Acad. Nya Handlingar, 1784, p. 218. An account of them will be found in the appendix to Crawford's *Treatise on Heat.*

From this table it appears that the real specific gravity of mixtures of alcohol and water, is in all cases above the mean, and that this excess is greatest when 1 atom alcohol is mixed with 3 atoms of water. The augmentation amounts in that case to almost $\frac{1}{27}$th part. It is evident that when alcohol and water unite, the atoms approach nearer each other than before. We might expect, therefore, that the specific heats would rather diminish in proportion as the specific gravity increases.

The following table exhibits the specific heats of these different mixtures, determined by measuring the time which given volumes of such mixtures took to cool a certain number of degrees; usually 40°.

			Sp. heats.	Mean specific heat by atoms.	Differences.	Mean sp. heat by weight.
Absolute alcohol .			0·6600			
Alcohol.		Water.				
4 atoms	+	1 atom	0·6775	0·7280	—0·0505	0·6903
3	+	1	0·7576	0·7451	+0·0126	0·6992
2	+	1	0·8034	0·8150	—0·0116	0·7156
1	+	1	0·8466	0·830	+0·0166	0·7576
1	+	2	0·9210	0·886	+0·034	0·8092
1	+	3	0·9915	0·915	+0·0765	0·8436
1	+	4	0·9962	0·932	+0·0642	0·868
1	+	5	1·0052	0·9433	+0·0619	0·8885
1	+	6	1·0204	0·9516	+0·0690	0·8984
1	+	7	1·0136	0·9575	+0·0561	0·9090
1	+	8	1·0227	0·9622	+0·0605	0·918
1	+	9	1·0398	0·966	+0·0738	0·9248
1	+	10	1·0277	0·969	+0·0587	0·9308

The specific heat in this table is considerably above the mean, except in two cases; but if we take into view the weight of the atoms of alcohol and water, we obtain the mean specific heats in the last column of the table. By both ways of calculating we see that the specific heats, with one or two exceptions, are above the mean. It is impossible, therefore, if any confidence can be placed in these experiments, to account for the heat evolved by a diminution of the specific heat, when, so far from diminishing, the specific heat in reality increases.

The heat produced by mixing alcohol and water is very small. 24 cubic inches of the alcohol were usually employed.

When an atom of (1 alcohol + 1 water) was mixed with 1 atom of water, the heat evolved was 7°. When an atom of water was added to an atom of (2 alcohol + 1 water), the temperature rose 4°. The same rise was observed when an atom of water was mixed with (1 alcohol + 2 water). Sect. III.

When 12 cubic inches of absolute alcohol were mixed with so much water as to constitute a mixture of (1 atom alcohol + 4 atoms water), the temperature rose 14°. When 12 cubic inches of (1 alcohol + 4 water) were mixed with such a quantity of water as to constitute (1 alcohol + 9 water), the rise of temperature was 7°.

It is obvious that the heat evolved when alcohol and water are mixed, cannot be owing to a diminution of the specific heat, since in most of the mixtures the specific heat is greater than the mean.

(2.) *Nitric Acid.*

A similar set of experiments was made upon mixtures of nitric acid and water. The strongest acid in my possession had a specific gravity of 1·504. It was a compound of 1 atom acid, and 1·37 atoms water.

The following table exhibits the specific gravity of mixtures of this acid and water in atomic proportions:—

Atoms Nitric Acid.	Atoms Water.	Sp. Gravity.	Mean do.	Differences.
1	+ 1·37	1·504		
1	+ 2	1·4862	1·4646	+ 0·0216
1	+ 3	1.4477	1·4321	+ 0·0165
1	+ 4	1·4177	1·4030	+ 0·0147
1	+ 5	1·4005	1·3961	+ 0·0044
1	+ 6	1·3724	1·3671	+ 0·0053
1	+ 7	1·3598	1·3268	+ 0·0023
1	+ 8	1·3235	1·3233	+ 0·0002
1	+ 9	1·3007	1·2986	+ 0·0021
1	+10	1·2815	1·2773	+ 0·0042

The specific gravity is in every case above the mean; and the greatest diminution of bulk is when 1 atom acid combines with two atoms of water.

Chap. I.

The following table exhibits the heat evolved when the acid and water are mixed in atomic proportions :—

Acid. Water.	Water.	Thermometer rises from	Heat evolved.
(1 + 1·37)	+ 0·63	63° to 112°	49°
(1 + 2)	+ 1	47 to 90	43
(1 + 3)	+ 1	46 to 75	29
(1 + 4)	+ 1	50 to 65	15
(1 + 5)	+ 1	60 to 78	18
(1 + 6)	+ 1	42½ to 61	18½
(1 + 7)	+ 1	68½ to 71½	3
(1 + 8)	+ 1	63 to 66	3
(1 + 9)	+ 1	68 to 70	2

The greatest evolution of heat takes place doubtless when (1 acid + 1 water) is mixed with an atom of water, corresponding to the greatest increase of specific gravity.

The following table exhibits the specific heats of these various mixtures :—

Acid.		Water.	Sp. Heat.	Mean do.	Differences
1	.	+ 1·37	0·4645		
1	.	+ 2	0·5138	0·5066	+ 0·0072
1	.	+ 3	0·5553	0·5691	— 0·0138
1	.	+ 4	0·5834	0·5997	— 0·0163
1	.	+ 5	0·6021	0·6214	— 0·0193
1	.	+ 6	0·6415	0·6352	+ 0·0063
1	.	+ 7	0·6495	0·6690	— 0·0195
1	.	+ 8	0·6832	0·6731	+ 0·0101
1	.	+ 9	0·6941	0·7043	— 0·0102
1	.	+ 10	0·7239	0·7228	+ 0·0011

Here also the specific heat of the mixture is sometimes greater and sometimes less than the mean. Of course it is impossible that the constant evolution of heat which is occasioned by every mixture, can be accounted for by a diminution of the specific heat.

(3.) *Sulphuric acid.**

The following table exhibits the specific gravity of mixtures of sulphuric acid and water in atomic proportions :—

* Records of General Science, iv. 252.

Acid.	Water.	Specific gravity.	Mean ditto.	Differences.
1 atom	+ 1 atom	1·8422		
1	+ 2	1·7837	1·7114	+0·0723
1	+ 3	1·6588	1·6158	+0·0430
1	+ 4	1·5593	1·5429	+0·0164
1	+ 5	1·4737	1·4854	—0·0117
1	+ 6	1·4170	1·4389	—0·0219
1	+ 7	1·3730	1·4006	—0·0276
1	+ 8	1·3417	1·3684	—0·0267
1	+ 9	1·3105	1·3410	—0·0305
1	+10	1·2845	1·3174	—0·0329

The first three of these mixtures have a specific gravity above the mean; but that of the last six is below the mean.

The following table exhibits the heat evolved when sulphuric acid of (1 acid + 1 water) is mixed with water in atomic proportion:—

Acid. Water.	Water.	Therm. rose from	Heat evolved.
(1+ 1)	+1	60° to 245°	185°
(1+ 2)	+1	65 to 135	70
(1+ 3)	+1	64 to 110	46
(1+ 4)	+1	60 to 95	35
(1+ 5)	+1	63 to 76	13
(1+ 6)	+1	63 to 72	9
(1+ 7)	+1	63 to 70	7
(1+ 8)	+1	63 to 69	6
(1+ 9)	+1	63 to 67	4
(1+10)	+1	63 to 66	3

The heat evolved was much greater when 1000 grains of oil of vitriol of sp. gr. 1·8422 were mixed with atomic proportions of water, as shown in the following table:—

Oil of vitriol.	Water.	Weight in grains of Acid.	Water.	Therm. rises from	Heat evolved.
1 atom	+1 atom	1000	183·6	60° to 245°	185°
1	+2	1000	367·3	67 to 286	219
1	+3	1000	550·9	60 to 268	208
1	+4	1000	734·6	60 to 263	203
1	+5	1000	918·3	60 to 238	178
1	+6	1000	1102	59 to 222	163
1	+7	1000	1285·7	59 to 207	148
1	+8	1000	1469·3	59 to 198	139
1	+9	1000	1653	59 to 188	129

The following table exhibits the specific heats of these mixtures of oil of vitriol and water:—

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Acid.	Water.	Sp. heats.	Mean do.	Differences.
1 atom	+ 1 atom	0·3593		
1	+ 2	0·4707	0·4587	+0·0120
1	+ 3	0·4786	0·5326	—0·0540
1	+ 4	0·5228	0·5869	—0·0641
1	+ 5	0·5690	0·6306	—0·0616
1	+ 6	0·6091	0·6660	—0·0569
1	+ 7	0·6424	0·6952	—0·0524
1	+ 8	0·6699	0·7197	—0·0498
1	+ 9	0·7003	0·7405	—0·0402
1	+10	0·7201	0·7581	—0·0384

Here we see also all the specific heats of these mixture below the mean, except the first. It is obvious from tables that Dr Irvine's explanation of the heat evolved liquids combine chemically, cannot be the true one. consequently, that his method of detecting the real zer this evolution cannot lead to correct results.

Let us now see how far the theory of specific heat, as it has stated in this section, will assist us in accounting for this evolu

It follows from the constant quantity obtained by multipl the atomic weight into the specific heat of simple bodies, each atom is surrounded by the same quantity of heat. law holds also with binary compounds; each atom retai the quantity of heat with which it was surrounded before bination. But when a great number of atoms unite toget this rule does not hold. A certain portion of the heat appears in consequence of the union, and this heat w escapes doubtless accounts for the heat evolved during combination. The following table exhibits the product the atomic weight of mixtures of sulphuric acid and wa multiplied into the respective specific heats:—

Mixtures. Acid.	Water.	Atomic weights.	Specific heats.	Product of cols. 2 and 3.	
1 atom	+ 1 atom	6·125	0·3593	2·201	=0·375× 6
1	+ 2	7·25	0·4707	3·412	=0·375× 9
1	+ 3	8·375	0·4786	4·008	=0·375×10
1	+ 4	9·5	0·5228	4·966	=0·375×13
1	+ 5	10·625	0·5690	6·046	=0·375×16
1	+ 6	11·75	0·6091	7·157	=0·375×19
1	+ 7	12·875	0·6429	8·277	=0·375×22
1	+ 8	14	0·6699	9·379	=0·375×25
1	+ 9	15·125	0·7003	10·592	=0·375×28
1	+10	16·25	0·7201	11·702	=0·375×31

last column shows to what number, multiplied by 0·375, product of the atomic weight, by the specific heat, is equal. ere can be no doubt that if the specific heats were absolutely rect, the multiples of 0·375 would be as follows :—

Acid.	Water.	Multiples.	Number of atoms.	Differences.
1 atom	+ 1 atom	6	7	—1
1	+ 2	9	10	—1
1	+ 3	11	13	—2
1	+ 4	13	16	—3
1	+ 5	16	19	—3
1	+ 6	19	22	—3
1	+ 7	22	25	—3
1	+ 8	25	28	—3
1	+ 9	28	31	—3
1	+10	31	39	—3

The third column of the table shows the number of atoms contained in each mixture.

It appears from this table, that when one atom of sulphuric acid and one atom of water combine, though the number of atoms in the mixture be 7, yet the product of the atomic weight into the specific heat is only 0·375 × 6; consequently, a seventh of the whole heat is dissipated. The consequence of this dissipation must be the evolution of an enormous quantity of heat, as is known to be the case when anhydrous sulphuric acid and water are mixed together.

When one atom of acid is mixed with three atoms of water, or when an atom of oil of vitriol is mixed with two atoms of water, the product of the atomic weight and specific heat is only 0·375 × 11, instead of by 13, so that $\frac{2}{13}$ths of the whole heat is evolved. But we have seen that in this case the heat evolved is 219°. But 219° is $\frac{2}{13}$ths of 1423°·5; so that the whole heat present before the mixture was 1423°·5. If we could determine the elevation of temperature, with accuracy, we have here a mode of coming to the real zero. But it is obvious that the heat evolved must exceed 219°. The thermometer rises with great rapidity, and begins to sink immediately, and a good deal of the heat is expended in the vessel in which the experiment is made.

It is obvious that the same mode of reasoning will apply to

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nitric acid and alcohol. The following table exhibits the re-quisite data for nitric acid :—

	Number of atoms.	Atomic weights.	Specific heats.	Product of cols. 3 and 4.	
1+ 1·37	10	8·29125	0·4625	3·851	=0·375×10·27
1+ 2	12	9	0·5138	4·624	=0·375×12·33
1+ 3	15	10·125	0·5553	5·562	=0·375×14·832
1+ 4	18	11·25	0·5834	6·563	=0·375×17·538
1+ 5	21	12·375	0·6021	7·451	=0·375×19·869
1+ 6	24	13·5	0·6415	8·660	=0·375×23·093
1+ 7	27	14·625	0·6495	9·581	=0·375×25·549
1+ 8	30	15·75	0·6832	10·760	=0·375×28·693
1+ 9	33	16·875	0·6941	11·713	=0·375×31·236
1+10	36	18	0·7239	13·030	=0·375×35·013

The reason of the small elevation of temperature, when nitric acid and water are mixed, becomes evident from this table.

We cannot apply this mode of reasoning to alcohol. The number of atoms is so great, and several circumstances connected with its atomic constitution are so uncertain, that our data are not sufficiently accurate for the purpose.

It is obvious that in all cases, when heat is evolved by mixing two liquids together there exists an affinity between them, and when mixed they enter into a chemical combination. Now it is very seldom that bodies unite chemically, without undergoing a change in their volume. Sometimes the bulk, after union, increases; but most commonly it diminishes. In the first case the atoms of which the bodies are composed, separate to a greater distance than before their union; in the second case they approach nearer each other. In the case of expansion we would expect a diminution of temperature; in the case of contraction an increase. There can scarcely be a doubt that these alterations in temperature take place in almost all chemical combinations, though other circumstances frequently accompany these combinations, which prevent us from perceiving the alteration of temperature produced by the combination. For example, when salts are dissolved in water, the density of the compound is almost always greater than the mean. Heat, therefore, should be evolved. But in most cases we are prevented from perceiving any augmentation of

Cold from solution of salts.

temperature by the liquefaction of the water of crystallization, which most salts contain. When the quantity of this water of crystallization is considerable, the heat necessary for its liquefaction is so great that a considerable diminution of temperature takes place. I select the following experiments, which I made many years ago, to satisfy myself what change in the real density of the compound takes place even in those salts which occasion sensible cold by their solution in water:—

Salt in crystals.	Sp. gravity of salt.	Salt dissolved.	In grains of water.	Sp. gr. of solution.	Mean gravity.*	Differences.
		Grains.				
Carbonate of soda	1·624	300	1000	1·0880	1·0973	—0·0093
Sulphate of soda	1·3497	299	1000	1·0908	1·0634	+0·0274
Sulphate of iron	1·8	300	1000	1·1313	1·1142	+0·0171
Sulphate of magnesia	1·862	300	1000	1·1206	1·1197	+0·0009

In all these solutions, a contraction takes place, except in the case of carbonate of soda, where there is an expansion. When these salts are dissolved in water, cold is produced; but the diminution of temperature depends as much upon the rapidity of the solution as upon any thing else. But the greatest cold is produced by the solution of carbonate of soda, in which there is expansion instead of contraction. The following table shows the diminution of temperature which takes place when 300 grains of each of the preceding salts in the state of powder, but retaining their water of crystallization, are thrown into 1000 grains of water and stirred with a thermometer as long as the temperature continues to sink:—

* The method of calculating the mean specific gravity of two bodies, or the specific gravity of a compound, supposing that no change of volume takes place, and that we know the specific gravity of each of the constituents is as follows:—

Let the two bodies uniting be A and B. Let S = specific gravity of A, W = its weight. S′ = specific gravity of B. W′ = its weight, then the mean specific gravity of the compound is $\frac{(W + W')\,S\,S'}{W\,S' + W'\,S}$.

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	Temperature of water before adding the salts.	Ditto after agitation.	Diminution of temperature.
Carbonate of soda	59°	43°	16°
Sulphate of soda	57·5	45·5	12
Sulphate of magnesia	56·5	51	5·5
Protosulphate of iron	58	53·5	4·5

Heat from anhydrous salts.

When these salts, in an anhydrous state, are thrown into water, the temperature rises. Thus 300 grains of anhydrous carbonate of soda, thrown into 1000 grains of water, and stirred, elevated the temperature of the solution from 57°·5 to 79°·5, or 22 degrees. The salt was nearly, though not completely, dissolved. The specific gravity of anhydrous carbonate of soda, is 2·640, and the specific gravity of a saturated solution at 80° of this salt, is 1·2291, and it is a compound of 1000 water + 292·3 salt, or at 80°, 100 water dissolved 29·23 of the anhydrous salt. In this case, there is a great diminution of volume; for the specific gravity, supposing no alteration in volume, should be 1·1636. The contraction amounts to 0·0555. Three hundred grains of anhydrous sulphate of soda being thrown into 1000 grains of water at 61°·5, the temperature rose to 65°·5 or 4°. It continued long at that temperature. Much of the salt remained undissolved. The specific gravity of the solution was 1·1549. It consisted of 1000 grains of water holding in solution 164·84 grains of anhydrous salt. The mean specific gravity is 1·0934. The volume therefore has contracted.

M. Kupffer has determined experimentally the change of volume which takes place, where lead and tin, mercury and tin, and mercury and lead, are alloyed in various proportions. He found the specific gravity of the metals which he employed as follows:—

Lead . . .	11·3308
Tin . .	7·2911
Mercury .	13·5886

at the temperature of 39°·38, the point of maximum density of water.

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Density of alloys of tin and lead.

Alloy of Tin.	Lead.	Specific gravity of the alloy.	Mean specific gravity of the two metals.	Differences.
1 atom	1 atom	9·4263	9·4366	—0·0103
1	2	10·0782	10·0936	—0·0154
1	3	10·3868	10·4122	—0·0254
1	4	10·5551	10·6002	—0·0431
2	1	8·7454	8·7518	—0·0064
3	1	8·3914	8·3983	—0·0069
4	1	8·1730	8·1826	—0·0096
5	1	8·0279	8·0372	—0·0093
6	1	7·9210	7·9326	—0·0116

The specific gravity is always less than the mean. These metals, therefore, dilate when they combine. The dilatation is least between the alloy of 2 atoms tin + 1 atom lead, and the alloy of 3 atoms tin + 1 atom lead. M. Kupffer found that an alloy of 2½ atoms tin + 1 atom lead, neither undergoes contraction nor dilatation, but is precisely the mean of that of the two metals alloyed.

The following table exhibits the specific gravity of the amalgams of tin:—

Of amalgams of tin.

Amalgams of Tin.	Mercury.	Specific gravity of alloy.	Mean specific gr. of metals.	Differences.
1 atom	1 atom	11·3816	11·3480	+ 0·0336
2	1	10·3447	10·2946	+ 0·0501
4	1	9·3185	9·2658	+ 0·0527
6	1	8·8218	8·7635	+ 0·0583

Here the specific gravity is always above the mean, so that the metals, when they unite, diminish in volume, and this diminution increases as the quantity of tin increases.

The following table exhibits the specific gravity of various amalgams of lead:—

Of amalgams of lead.

Volumes of Lead.		Mercury.	Specific gravity.	Mean gravity.	Differences.
1	+	4	13·1581	13·1116	+ 0·0465
1	+	3	13·0397	13·0003	+ 0·0394
1	+	2	12·8648	12·8147	+ 0·0501

Here also the volume diminishes when the two metals com-

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The alteration in volume which takes place when bodi unite together, and the quantity of heat evolved, or absorbe during the union, when studied with more accuracy than the have hitherto been, will doubtless furnish most important dat for investigating the nature of affinity, and for determining th difference in the intensity of this force in different combination I shall therefore give a few more examples.

1. The combinations of the metals with sulphur are a definite, and generally intimate. In all cases hitherto exami ed, these combinations are accompanied by a diminution bulk, which in some cases is very considerable. The follo ing table exhibits the composition and specific gravity of som of these sulphurets:—

Metals.	Sp. gr.	Atomic weights.
Iron	7·8	3·5
Zinc	6·861	4·125
Lead	11·357	13
Copper	8·9	4
Bismuth	9·833	9
Silver	10·474	13·5
Mercury	13·568	12·5
Sulphur	2·086	2

Of sulphurets.

Names of sulphurets.	Constituents. Metal.	Sulphur.	Sp. gr.	Ditto by calculation.	Difference
Protosulphuret of iron	1 atom	+ 1 atom	4·518	4·344	0·174
Bisulphuret of iron	1	+ 2	4·83	3·1695	1·6605
Sulphuret of zinc	1	+ 1	4·044	3·960	0·084
Sulphuret of lead	1	+ 1	7·602	7·115	0·487
Disulphuret of copper	2	+ 1	5·792	5·383	0·309
Sulphuret of bismuth	1	+ 1	7·591	6·647	0·943
Sulphuret of mercury	1	+ 1	7·780	7·730	0·056
Sulphuret of silver	1	+ 1	7·2	6·9176	0·2824

* Ann. de Chim. et de Phys. xl. 286.

The most remarkable of these compounds is the bisulphuret of iron, which is composed of 1 atom iron + 2 atoms sulphur. When iron combines with sulphur, atom to atom, the specific gravity is considerably above the mean, being 4·518. Now, when sulphur, which has a specific gravity of 2·086, combines in a sulphuret having a specific gravity more than double, one would expect that the specific gravity of the compound should diminish. It comes out by calculation only 3·4465: but is really 4·83; so that the specific gravity has increased almost two-fifths. Sect. III.

2. Oxygen being in a gaseous state and metals solid, we cannot so well determine the diminution of bulk which takes place when they combine. Yet as the oxygen is condensed into a solid in all of these compounds, we have it in our power at least to observe the comparative condensation, which may suggest some useful ideas. I shall therefore give the following examples of these combinations:—

	Oxides.	Composition.		
1	Protoxide of lead . .	13 lead	+	1 oxygen
2	Red lead	13	+	1·5
3	Peroxide of lead . .	13	+	2
	Peroxide of tin . . .	7·25 tin	+	2
1	Red oxide of copper . .	8 copper	+	1
2	Black oxide of copper .	4	+	1
	Oxide of bismuth . .	9 bismuth	+	1
1	Black oxide of mercury .	25 mercury	+	1
2	Red oxide of mercury .	25	+	2
	Oxide of silver . . .	13·5 silver	+	1
	Chloride of silver . .	13·5	+	4·5 chlorine
	Oxide of nickel . .	3·25 nickel	+	1 oxygen.

If we reckon the specific gravity of water unity, the specific gravity of oxygen gas and chlorine gas referred to water is:

Oxygen . . .	0·0013723
Chlorine . .	0·00308325

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Of oxides.

OXIDES.	Sp. gr.	Sp. gr. by calculation, supposing no condensation.	Density, supposing the calculated density to be 1.
Protoxide of lead .	9·277	0·019136	48·469
Red lead . . .	9·096	0·013068	69·605
Peroxide of lead .	8·902	0·010261	86·755
Peroxide of tin . .	6·97	0·006342	109·90
Red oxide of copper .	6·093	0·012308	49·619
Black oxide of do. .	6·401	0·006857	93·477
Oxide of bismuth .	8·211	0·013674	60·049
Black oxide of mercury	10·69	0·035590	30·036
Red oxide of do.	11·085	0·018502	59·913
Oxide of silver . .	7·143	0·020158	35·435
Chloride of do. . .	5·129	0·012436	41·243
Peroxide of nickel .	4·846	0·005829	83·133

The last column of this table enables us to compare the increase of density which these combinations undergo with each other. We observe, contrary to what might have been expected, that the density increases with the quantity of oxygen added. This holds in the case of lead, copper, and mercury. It held also in the case of sulphur, in the former table. It would seem then to be a general law, that the diminution of volume increases in proportion to the quantity of the lightest substance added to the compound. Yet these additional doses of oxygen are more easily expelled by heat, than the portion which constitutes the protoxide.

The increase of density differs in every one of these examples. It is greatest in the peroxide of tin, and black oxide of copper, which are both difficult of decomposition by heat; though I have converted black oxide of copper into red oxide by an intense heat. The most easily decomposed oxide in the table, is the black oxide of mercury, and its increase of density is the least. Red oxide of mercury resists decomposition better, and

its increase of density is greater. Oxide of silver is also easily decomposed by heat, and its increase of density is comparatively low.

We ought to be cautious in drawing conclusions from such tables, till the specific gravity of the metallic oxides be determined with more precision than has hitherto been done. I have observed that the specific gravity of black oxide of copper increases the oftener it has been exposed to a red heat. The same thing perhaps may hold with the other oxides; such of them at least as may be exposed to such a process.

Irvine's explanation of liquidity.

3. There is another general principle respecting the specific heat of bodies, which was investigated with much ingenuity by Dr Irvine, and which deserves to be explained in consequence of the great importance attached to it, and the many ingenious theories founded upon it. According to Dr Irvine, when a body changes its state, its specific heat changes at the same time, and it changes according to a law, which may be thus expressed —when a *solid* becomes a *liquid*, its specific heat *increases*, when a *liquid* becomes an *elastic fluid*, its specific heat increases. On the other hand, when an *elastic fluid* is converted into a *liquid*, its specific heat *diminishes*, and when a *liquid* is converted into a *solid*, its specific heat *diminishes*.

I am not aware that any attempts have hitherto been made to establish the accuracy of this law, except in the case of water, and the results are so discordant that little confidence can be placed in them. Water, as is well known, exists in three states. 1. That of a solid, when it is called *ice*. 2. That of a liquid, when it is called *water*. And, 3. That of a gaseous fluid, when it is called *steam*.

(1.) The specific heat of ice has been determined as follows:—

By Clement and Desorme . .	0·72
By Dr Irvine	0·8
By Avogadro	0·92
Mean	0·813

(2.) The specific heat of water is 1.

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(3.) The specific heat of steam:—

By Delaroche and Berard . .	0·8470
By Avogadro	1·225
By Crawford	1·55
Mean	1·207

These experiments, the best we have, give as the specific heat of

Ice . . .	0·813
Water . . .	1·000
Steam . . .	1·207

The law then holds with respect to that substance. But it would be dangerous to generalize farther, till it has been determined experimentally whether the rule holds in other substances besides water. Sulphur, and such of the volatile oils as become solid when exposed to moderate cold, might be examined without any insuperable difficulty.

SECTION IV.—OF THE CELERITY OF THE COMMUNICATION OF HEAT.

Heat enters and leaves bodies with the same velocity.

1. It is generally admitted that heat is disposed to enter into, or to leave the different kinds of matter with the same degree of celerity. This is the foundation of the mode of determining the specific heat of bodies, by the time which they take in cooling. For it is obvious, that unless heat separate from every body, with the same celerity, we never can judge of the quantity of heat which each contains under the same temperature by this method. But if heat leave every body with the same celerity, then that body will be the longest in cooling which contains the most heat, and consequently has the greatest quantity to lose. In this way, the specific heats of various metals, woods, and liquids, have been determined, and the results agree with experiments made in a different way. So far as we know at present, there is no exception to this general law.

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Conduction of heat through bodies.

2. When a body of some length is applied to a source of heat by one extremity, while a thermometer is attached to its other extremity, we shall find that a certain portion of the heat will pass through the body to the thermometer, and cause it to rise. When heat passes in this way, it is said to be *conducted* through the body. Now, if we try various substances in this way, we shall find a great difference in the quantity of heat which they allow to pass. Some under such circumstances speedily become hot, and transmit a great deal of heat, while in others, the temperature rises comparatively less, and they transmit less heat to the thermometer. The first kind are said to be *good conductors* of heat; the second kind are called *bad conductors*. The metals are the *best* conductors among solid bodies. Stones, bricks, and earths, are much inferior in this respect; and liquids, and elastic fluids, when their particles are prevented from moving, or when their mobility is greatly retarded, are exceedingly *bad* conductors; but they are *good* conductors when they are at perfect liberty to move from the hot to the cold body.

There is a sensible difference between the conducting power of different metals, even when placed as nearly as possible in the same circumstances. If the extremity of a silver wire, a few inches in length, be held in the flame of a candle, the other end soon becomes so hot that it burns the fingers; whereas a platinum wire, of the same length, may be held in the flame of a candle for any length of time without producing that effect.

Experiments of Ingenhousz.

A method of determining the relative conducting powers of the metals was contrived by Dr Franklin, and executed by Dr Ingenhousz, in the year 1780, which, though it does not give any very exact results, enables us at least to form some notions of the relative intensities of each. Wires of gold, silver, copper, tin, steel, iron, and lead, of exactly the same size and length, were fixed at equal distances in a piece of wood, through which they passed perpendicularly. The portion of the wires above the wood (which was the most considerable) was covered with a coat of white wax by dipping them in melted wax. That part of the wires below the wood was now plunged into olive oil heated nearly to 212.° That

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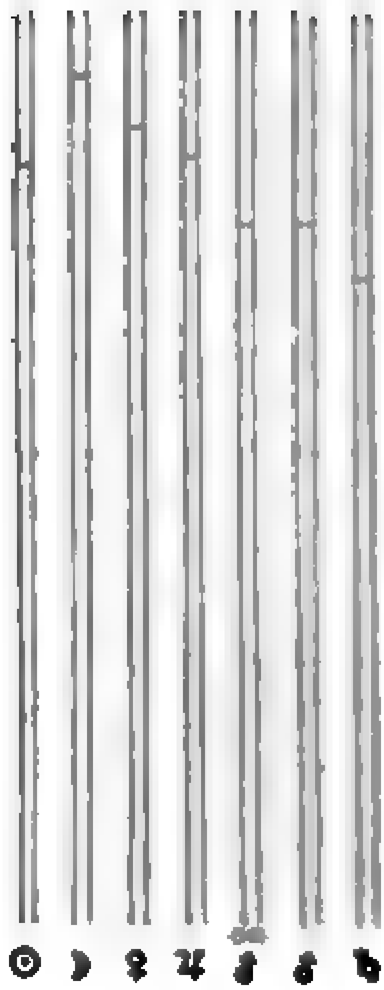
wire was considered as the best conductor, which melted the wax highest up. Twelve experiments were made. The figure in the margin represents the wires above the wood in one of these experiments, and the cross lines show how high the wax was melted in each. In all the experiments the wax was melted highest up on the silver wire, next highest on the copper wire, and lowest of all on the leaden wire. In the others it varied. But gold and tin seem to stand next to copper, then comes iron, and then steel.



So that, according to these experiments, the conducting power of the seven metals tried is in the following order:—*

Silver,
Copper,
Gold, }
Tin, }
Iron, }
Steel, }
Lead.

Of Despretz.

But the most interesting set of experiments on this subjec has been made by M. Despretz. He employed bars of th different substances of the same size, and covered with a coat ing of varnish. One of the ends of the bar was heated by lamp, while a thermometer was applied to the other. Th height to which the thermometer rose after the lamp had ex hausted its effect, measured the conducting powers of th bodies. The following table exhibits the conducting power of the various substances tried by Despretz, according to th results of his experiments:—†

* *Nouvelles Experiences*, par Dr Ingenhousz, p. 380.

† Ann. de Chim. et de Phys. xix. 97.

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	Conducting power.
Gold . . .	100
Platinum . . .	98·1
Silver . . .	97·3
Copper . . .	89·82
Iron . . .	37·41
Zinc . . .	36·37
Tin . . .	30·38
Lead . . .	17·96
Marble . . .	2·34
Porcelain . . .	1·22
Brick earth . .	1·13*

It is probable that this difference in the rate in which heat passes through bodies is connected with the closeness of their texture, or at least with the contiguity of the particles of which they are composed. A set of experiments made by MM. Aug. Delarive and Alph. de Candolle corroborates this view of the subject.† They took pieces of dry wood, about $5\frac{1}{8}$ inches long, $1\frac{1}{2}$ inch broad, and 1 inch thick. At about $1\frac{1}{8}$ inch from the extremity of these pieces of wood, a hole was bored, reaching to the centre of the piece, and rather more than $\frac{1}{4}$th inch in diameter. Five such holes were made in each piece, at the distance of rather more than $\frac{3}{4}$ths of an inch from each other. Into each of these holes was put a little mercury, into which the bulb of a thermometer was plunged. Over the mercury was strewed a little lycopodium powder, to prevent radiation of heat from the mercury. One of the extremities of the piece of wood was inserted in a case of tin plate, about $\frac{3}{4}$ths of an inch long, so as not to cover any of the holes. This apparatus was suspended freely in the air; and a spirit lamp was placed under the extremity covered by the tin plate. The flame was prevented from striking against any other part of the wood by the chimney of the lamp, and by pieces of glass placed vertically between it and the wood, which were carefully renewed whenever the heat began to pass through them. Care was taken to regulate the heat so as to prevent all risk

Of Delarive and Decandolle.

* Traité Elementaire de Physique, par M. Despretz, p. 201.

† Ann. de Chim. et de Phys. xl. 91.

Chap. I. of injuring the texture of the wood. Some of these piec
wood were cut, so that the woody fibres extended longitudin
or in the direction of the wood. Others were in the cont
direction, so that the length of the piece was in the direc
from the pith to the bark of the tree.

The heat was continued in each case for about a quarte
an hour after all the thermometers had ceased to ascend. '
following table exhibits the rise of the several thermome
in these experiments above the temperature of the roon
which the experiments were conducted.

I. The woody fibres being longitudinal, or in the direct
by which the heat flowed.

Names of the woods.	Centigrade Thermometer.				
	1st therm.	2d therm.	3d therm.	4th therm.	5th therm.
Cratægus aria	83°	45°	21°·2	9°·2	4°·4
Nut wood	80·13	43	19·63	9·19	5·13
Oak	81·7	41·2	17·5	7·2	3·7
Fir	84	39·25	20·6	8·5	3·7
Poplar	79·8	34·2	14·2	6·2	2·8

II. The heat flowing in a direction opposite to that of woody fibres or across them.

Names of the woods.	Centigrade Thermometer.				
	1st therm.	2d therm.	3d therm.	4th therm.	5th therm.
Nut wood	99°·5	37°·43	13°·19	6°	3°·25
Oak	79·3	22·75	7·5	3·6	2·4
Fir	70·9	13·8	4·5	2·5	1·9
Cork	78·5	13·75	3·44	1·56	1

The following table exhibits the height to which the sec
thermometer would have risen, on the supposition that the 1
thermometer in every case rose 100° above the temperatur
the atmosphere.

I. Woody fibres longitudinal.

Cratægus aria	54·28
Nut wood	53.7
Oak	50·5

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Fir	47·62
Poplar	42·91

II. Heat flowing across the woody fibres.

Nut wood	37·59
Oak	28·57
Fir	19·6
Cork	17·5

This table would give the relative conducting powers of the different woods, if their specific gravity and power of radiating heat were the same in all. But this not being the case, it affords only an approximation.

In general, we see that the hardest woods conduct heat best; though this is not accurately true, as nut wood conducts rather better than oak, though the latter is the hardest of the two.

We see how much better wood conducts heat in the direction of the woody fibres than across them; and this difference increases with the badness of the wood as a conductor. The conducting powers in the two directions may be represented very nearly by the following numbers:—

	Longitudinally.	Across the fibres.
Nut wood	5	3·46
Oak	5	2·83
Fir	5	2·05

This inferiority in the conducting power of wood, when the heat moves perpendicularly to the direction of the woody fibres, will enable us to explain the reason why the temperature in the interior of the trunk of a tree, is nearly that of the soil from which they draw their nourishment.

The only other set of experiments on this subject with which I am acquainted, was made by Dr Trail, professor of medical jurisprudence in Edinburgh, on various liquids. The liquids under examination were put into a wooden cylinder, four inches long and two inches in diameter. A delicate thermometer was fixed so that its bulb was in the axis of this cylinder, at some distance from its bottom. An iron cylinder was heated for 15 minutes, in boiling water, and then intro-

Of Dr Trail.

Chap. I.

duced through a hole in the lid of the wooden box, till i tremity was exactly half an inch from the bulb of the mometer. The time which the thermometer took to rise degrees was marked by a stop-watch, when the cylinder tained different liquids. The following table exhibit results obtained:—

Liquids.	Time of heating 3°.	
Water	7′	5″
Cow's milk	8	25
Proof spirit	8	
Alcohol (0·815)	10	45
Olive oil	9	50
Mercury	0	15
1 Sulphate of iron, 5 Water	8	
Saturated solution of alum	9	40
Ditto of sulphate of soda	6	30
Solution of potash (Pharm. Lond.)	8	15*

Passage of heat through liquids depends on the expansion.

One would, at first sight, be disposed to conclude, tha conducting power of these liquids is inversely as the necessary to cause the thermometer to rise three de Were that supposition well founded, mercury would be the and alcohol the worst conductor of all the liquids tried. if we consider the way in which the experiments were we must be sensible, that the rise of the thermometer chiefly influenced by the change of density produced i liquid, by augmenting its temperature. Those liquids, specific gravity diminishes most rapidly when heated, allow the least heat to pass through them; because the ticles, as they receive heat, would ascend most rapidly surface. Accordingly, alcohol, which undergoes the gr expansion from heat, was the liquid which allowed the h pass downwards most slowly. While mercury, whose d undergoes an insignificant augmentation from the addit three degrees of heat, allowed the heat to pass through it more rapidly than any of the other liquids. To make satisf experiments on the conducting powers of liquids, it wo

* Nicholson's Jour. xii. 133.

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necessary to guard against this mobility of their particles, which opposes such an obstacle to the downward motion of heat through them, that Count Rumford was of opinion, that it prevented it altogether, till the contrary was shown by the experiments of Dr Murray and my own.

Passage of heat through gases.

Experiments have also been made by Dalton and Davy, upon the time that a thermometer takes to cool a certain number of degrees in different gases.

The following table shows the results: the first column gives the time a thermometer took to cool 15° or 20° by Dalton's trials; the second column the time a thermometer took to sink for each gas, from 160° to 106°, in 21 cubic inches of each gas in Davy's trials :—

Gases.	Time of Cooling.	
Chlorine . . .		. 186″
Carbonic acid . .	112″	. 165
Sulphuretted Hydrogen	100 +	. ——
Nitrous oxide . .	100 +	. 150
Olefiant gas . .	100 +	. 75
Common air . .	100	. 129
Oxygen . . .	100	. 107
Azotic gas . .	100	. 90
Nitrous gas . .	90	. —
Gas from pit coal .	70	. 55
Hydrogen gas . .	40*	. 45†

These two columns do not agree very well with each other, owing probably to diversities in the mode of conducting the trials; but we see in general, that bodies cool slowest in the heaviest, and most rapidly in the lightest gases. It is obvious that the rate of cooling depends chiefly upon the mobility of the gaseous particles. The process is different from the conduction of heat by solid bodies.

Mr Graham has shown, by a set of very decisive experiments, that the mobility, or the rate of diffusion of different gases, upon which the rate of cooling in them obviously de-

* Dalton's New System of Chem. i. 117.

† Phil. Trans. 1817, p. 60.

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pends, is inversely as the square root of the specifi
of each gas. The following table exhibits the resu
experiments compared with theory :—

	Rate of Diffusion.		
	By experiment.	By theory.	Spe vity
Hydrogen	3·83	3·7947	0
Carburetted hydrogen .	1·344	1·3414	0
Olefiant gas . . .	1·0191	1·0140	0
Carbonic oxide . .	1·0149	1·0140	0
Azotic gas . . .	1·0143	0·0140	0
Oxygen . . .	0·9487	0·9487	1
Sulphuretted hydrogen .	0·95	0·9204	1
Protoxide of azote . .	0·82	0·8091	1
Carbonic acid . .	0·812	0·8091	1
Sulphurous acid . .	0·68	0·6708	2

Rate at which bodies cool.

3. Heat is communicated from hot bodies to colder much the greater celerity the greater the difference the temperature of the two bodies is. Hence, whe ball is heated to redness and suspended in the air, i very great deal of heat during the first few instants quantity lost in an instant diminishes at a great ra ball approaches the temperature of the air in wh suspended.

Hypothesis of Newton.

Sir Isaac Newton was of opinion, that the qua heat lost in given small times, are proportional to th of heat remaining in the hot body, or to the difference its temperature and that of the air; and therefore, quantities of heat lost in equal divisions of time con series of proportionals, or form a geometrical pro From a set of experiments on the cooling of merc water, published in 1750, by Richmann,* it appears Newtonian law holds pretty nearly at as high temper 120°. But beyond that point the verification does r to have been attempted.†

* Novi Commentarii Acad. Scien. Imper. Petropol. tom. iii.

† Novi Comment. Acad. Scient. Imperalis Petropolitanæ, From Richmann's experiments in this paper, on the rate of water, mercury, alcohol, naphtha, oil of turpentine, and linse might have deduced the difference in the specific heats of bo this idea does not seem to have occurred to him.

Experiments of Martine.

Dr Martine, of St Andrews, in an *Essay on the Heating and Cooling of Bodies*, published in 1739, showed that the Newtonian law is not strictly accurate even at pretty low temperatures; and that if it were so, bodies would take an infinite time to cool down to the temperature of the surrounding medium. He endeavoured, both from a number of experiments made by Muschenbroek, and by some of his own, to show that the decrements of heat were partly equable and partly in proportion to the subsisting heats. But the bodies experimented on were never raised to a higher temperature than 112°—a point too low to enable him to deduce the true law of cooling. Yet Dr Martine had the merit of first pointing out the inaccuracy of the Newtonian law.

Of Erxleben.

Erxleben demonstrated, by decisive experiments, in a dissertation published in the eighth volume of the *Nova Commentaria* of the Gottingen Society, that the deviation of the Newtonian law from the truth increases rapidly as the temperature increases, and that we should fall into very great errors if we extended the law much beyond the temperatures at which it has been verified.

Of Delaroche.

M. De Laroche, of Geneva, in a paper on some properties of radiant heat, published in 1812,* pointed out the same deviation; though he does not seem to have been aware of the previous experiments of Erxleben. From his experiments it would appear, that the Newtonian law holds tolerably near at temperatures below 212°, but the error increases as the temperature augments, and at last becomes very great.

Of Dulong and Petit.

The prize dissertation of Dulong and Petit on the measure of temperature, and the laws of the communication of heat, published in 1818, contains an elaborate investigation of the law of cooling. From their experiments, it is sufficiently obvious, that at temperatures above 212°, the rate of cooling deviates enormously from the Newtonian law. The exhibition of a single set of their experiments will be sufficient to demonstrate this. The following table exhibits the rate of cooling of a mercurial thermometer in a vacuum: the first

* Annals of Philosophy, ii. 100.

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column represents the temperature according to the centigrade scale; the second column gives the degrees of heat lost per minute at the corresponding temperatures, supposing the rate of cooling for a minute to be equable:—

240°	10°·69
220	8·81
200	7·40
180	6·10
160	4·89
140	3·88
120	3·02
100	2·30
80	1·74

Were the Newtonian law accurate, the velocity of cooling at 200° ought to be twice that of 100°: we see from the table that it is more than thrice. The velocity of cooling at 240° ought to be thrice that at 80°: we see from the table that it is more than six times as great.*

I shall now endeavour to state the principal facts respecting the cooling of bodies. For our knowledge of them we are almost entirely indebted to the dissertation of Dulong and Petit, which has been just referred to.

(1.) When a body cools in a vacuum, the heat which it loses is owing entirely to radiation. When it cools in any kind of air, the process goes on more rapidly, because the quantity of heat radiated is the same as in vacuo, while an additional quantity of heat is conducted away by the air or gas.

(2.) The rate of cooling of a liquid confined in a vessel, is not altered by the size nor by the shape of that vessel.

Law of cooling.

(3.) The velocity of cooling of a thermometer in vacuo, for a constant excess of temperature, increases in a geometrical progression, when the temperature of the surrounding medium increases in an arithmetical progression. The ratio of this geometrical progression is the same, whatever be the excess of temperature considered. The truth of this law will be evident from the following table:—

* Annals of Philosophy, xiii. 242.

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Excess of temp. of the therm. centigr.	Velocity of cooling Water at 0°.	Ditto Water at 20°.	Ditto Water at 40°.	Ditto Water at 60°.	Ditto Water at 80°.
240°	10°·69	12·40	14·35	—	—
220	8·81	10·41	11·98	—	—
200	7·40	8·58	10·01	11·64	13·45
180	6·10	7·04	8·20	9·55	11·05
160	4·89	5·67	6·61	7·68	8·95
140	3·88	4·57	5·32	6·14	7·19
120	3·02	3·56	4·15	4·84	5·64
100	2·30	2·74	3·16	3·68	4·29
80	1·74	1·99	2·30	2·73	3·18
60		1·40	1·62	1·88	2·17

If we compare the five last columns of this table with each other, we shall find that the mean ratio is 1·161.

In general, this geometrical progression requires to be diminished by a constant quantity, in consequence of the heat radiated back to the hot body from the walls of the vessel in which it is cooling.

(4.) When a body cools in vacuo, the time of cooling is materially influenced by the nature of the surface of the hot body; those bodies cooling soonest which radiate best. But this difference does not affect the law of cooling in vacuo.

Communication of heat depends on the extent of surface touching.

(5.) When a body cools in any gas, the same portion of its heat is carried off by radiation that would be dissipated if it were cooling in a vacuum. Another portion is conducted off by the gas. This last portion is not affected by the nature of the surface of the hot body. It depends upon the conducting power of the different gases.

4. The celerity with which heat is communicated from hotter bodies to colder ones, when all other things are equal, is proportional to the extent of contact and closeness of communication between the bodies.

Every person is disposed to admit this law from general experience. It is obvious enough, that if we have two cubes and two spheres of iron, if we heat one of each sort and lay it on the other, the heat would be communicated much faster from the hot cube to the cold one, than from the hot sphere to the cold one.

When the masses which we compare are of the same shape,

Chap. I. but differ only in size, the smaller will cool in a shorter ti than the larger. Suppose two cubes of gold, the one 1 in in diameter, and the other 2. The smaller cube will c sooner than the larger, if both be placed in the same circu stances; because the surface of the larger cube is only f times greater than that of the smaller, while the quantity matter in it is eight times greater.

This general principle enables us to see why heat pas very slowly through those bodies which have a rough a spongy texture. Wood transmits heat more slowly than me on this account. Cork still more slowly than wood. Wo feathers, or furs, exceed even cork in the slowness with whi heat passes through them. It is indeed true that the inte stices of these bodies are filled with air; but as this fluid in a state of stagnation, and the particles at some distan from each other, it gives little assistance, or rather offers strong resistance to the transmission of heat. This is t reason why such materials are so effectual in keeping o bodies warm in cold weather. They may be equally employ to prevent heat from entering into bodies. If we wish to pr vent a lump of ice from melting in a warm place, we cann do better than wrap it in plenty of flannel or furs, or the li materials, which produce the wished-for effect, by retardi the communication of heat to the ice from the air and t other bodies in the neighbourhood.

Fluids transmit heat more rapidly than solids.

This retardation of the communication of heat does n take place in fluids equally as solids. Fluids receive he and transmit it generally more rapidly than solids. Of t we have an obvious example in the air: which, though ve rare and light, cools bodies exposed to it very fast. When hot body is plunged into water, it cools much sooner than would do if plunged into a mass of sand or iron filings. T difference is owing to the easy mobility of the particles

And why.

fluids. When any portion of a fluid receives heat, it becom specifically lighter, and of course ascends; another portion cold fluid supplies its place, is heated in its turn, becom dilated and ascends: and this renewal of cold particles will co tinue till the temperature of the hot body is sufficiently reduc

This ascending motion of *air*, when a lump of red hot iron is suspended in a room to cool, becomes evident when we hold our hand perpendicularly over the iron at some distance above it. Heat, by rarefying air, and thereby producing a motion in it, is more quickly and equably dispersed through it, than through solid matter. Were air destitute of this property, that portion of it nearest the surface of the earth would soon become unfit for breathing, and would prove destructive to all the inhabitants of our planet. It is the mobility and expansibility of air that preserves it in a state fit for living beings.

Air becomes colder as we ascend.

But there is a circumstance which at first sight seems inconsistent with this property of air, and which therefore requires to be explained; I allude to the unequal distribution of temperature in the atmosphere. The part of the atmosphere contiguous to the surface of the earth is known to be hottest. As we ascend to a more elevated region, the air gets colder and colder; till at a certain height above the level of the sea, even in the torrid zone, we come to the region of perpetual congelation. The summits of the Andes, some of which are elevated more than 21,000 feet above the level of the sea, are perpetually covered with snow, even under the equator. The Himalaya mountains are higher, and lie to the north of the tropic of Cancer; we need not be surprised, therefore, that their summits are covered with eternal snow. As we advance from the equator towards the poles, the line of perpetual snow, which at the equator is elevated 14,760 feet above the level of the sea, gradually approaches nearer the earth, till in latitude 70° it is only 2,834 feet above the level of the sea. The following diagram will give a better idea of the rate at which this height declines than could be obtained by the most elaborate description:—

Line of perpetual congelation.

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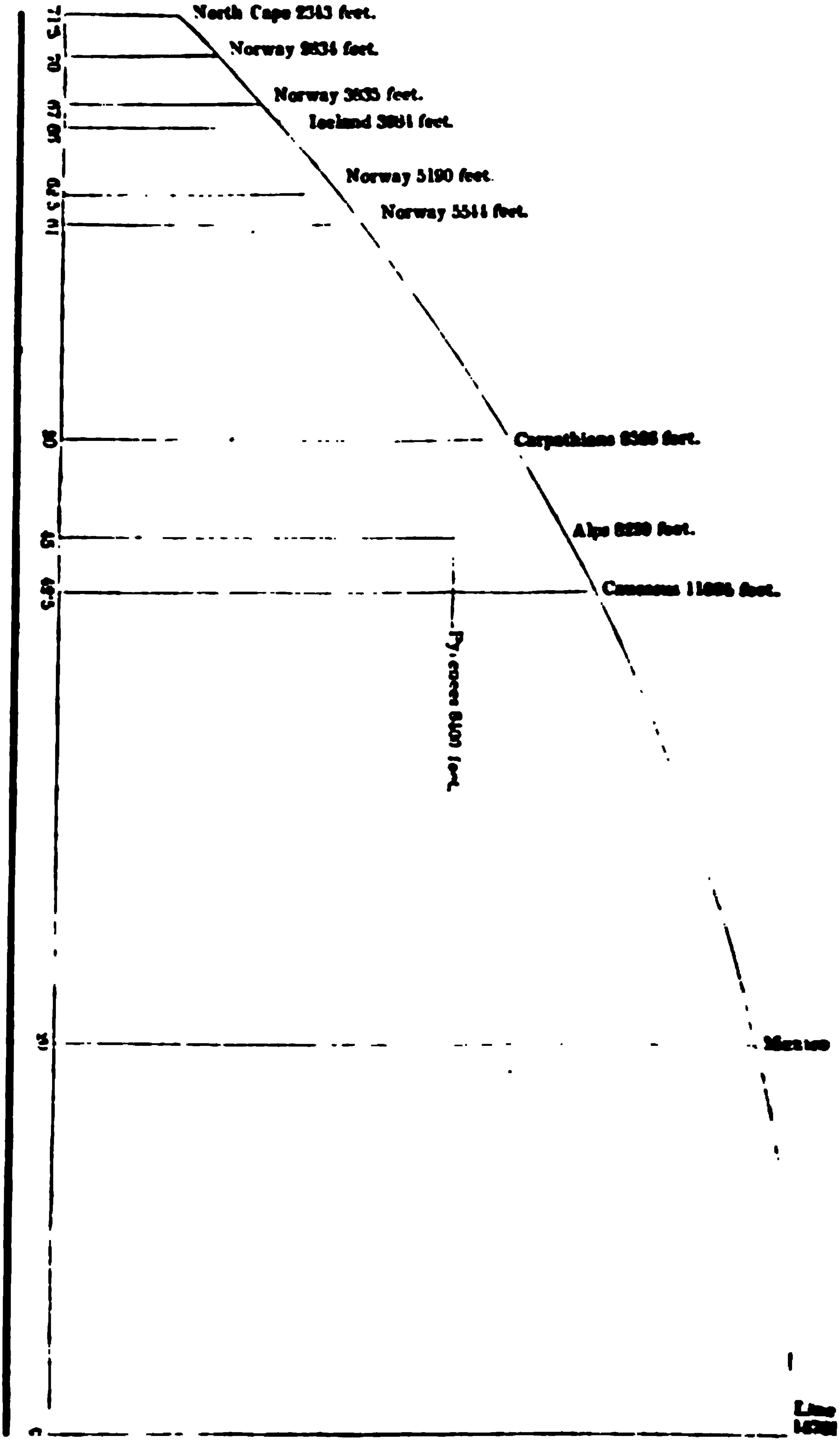

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The surface of the earth, instead of a quadrant of a circle, which is nearly its true form, is represented as a straight line on which the degrees of latitude are marked, beginning at the line and proceeding to N. latitude 71°·5 on the North Cape. The perpendicular height above the surface of the earth, representing the limit of perpetual congelation, is exaggerated about 1000 times.

From inspecting this diagram, it will be evident that the snow line, though connected with the latitude, is not absolutely regulated by it. Thus, in the Caucasus, in about latitude 42°½ N., the snow line is situated about 11,084 feet above the level of the sea; while in the Pyrenees, under the same latitude, it is only 8400 feet. In the Alps, it is 8220 feet above the level of the sea; while in the Carpathian, though situated a little farther north, it is 8526 feet or 326 feet higher. In Norway, in latitude 67°, it is 3835 feet above the level of the sea; while in Iceland, in latitude 66°, it is only 3084.

These apparent anomalies are easily accounted for. Whereever an extensive table-land occurs, its effect becomes manifest upon the atmosphere, and elevates the line of congelation considerably. Thus Mexico constitutes a table-land, elevated about 8000 feet above the level of the sea: the consequence is, that the snow line is only 660 feet lower than under the equator. This is the reason why the snow line is so high in the Himalaya mountains.

Wherever extensive glaciers are formed, they have a tendency to sink the temperature of the country, and by gradually creeping lower down, they depress the line of congelation in that place: hence the reason why the line of congelation is lower in the Alps than the Carpathians. In the Doffrines, there are glaciers in one place only: hence the reason why the line of congelation is higher in Norway than in Iceland. Various methods of calculating the height of the line of congelation have been proposed by ingenious philosophers of our own time; but it is obvious that no general rule can apply to all cases.

Because its specific heat increases with its volume.

Now, if air, the moment it is expanded by heat, begins to ascend to a higher part of the atmosphere, and if it continue

Chap. I. to rise till it come to air of the same density with itself, how are we to account for this regular diminution of heat, as we ascend? It would appear at first sight, that the higher parts of the atmosphere ought to be as hot, if not hotter, than those portions near the surface of the earth. The reason why this is not the case, is that the *specific heat* of air increases in proportion as its volume increases. But it is well known, that the higher from the surface of the earth any portion of the air is, the greater is its volume; for the volume of air is always inversely as the pressure upon it. At the surface of the sea, the incumbent air is pressed upon by the whole atmosphere above it. As we ascend, the air is pressed upon by all the portion of atmosphere above it, while the portion below ceases to have any action on it. The density of air continually decreases as we ascend in the atmosphere, and of course its bulk continually augments. The rate at which this augmentation of volume takes place will be obvious from the following table:—*

Height in miles above the sea.	Air. Density.	Volume.	Height of Barometer.
0	1	1	30 inches.
2·705	$\frac{1}{2}$	2	15
5·41	$\frac{1}{4}$	4	7·5
8·115	$\frac{1}{8}$	8	3·75
10·82	$\frac{1}{16}$	16	1·875
13·525	$\frac{1}{32}$	32	0·9375
16·23	$\frac{1}{64}$	64	0·46875

We see that when the height increases in an arithmetical ratio, the volume increases in a geometrical ratio.

Now, it has been experimentally ascertained, that as the volume of air increases, its specific heat augments in the same proportion. If a receiver, standing over the plate of an air-pump, and having a thermometer suspended in it, be exhausted, the thermometer suddenly sinks a few degrees, and then gradually rises again, till it acquires the temperature of the room in which the air-pump stands. When the air is again let into the receiver, the thermometer as suddenly rises a few

* When the barometer (at 32°) falls 0·001 inch, it indicates an elevation of 0·954 feet; so that the elevation of 1 foot causes the barometer to fall (at 32°) 0·00105 inch, according to the experiment of General Roy.

degrees, and then subsides again. Mr Dalton concluded from his observations, that the true elevation of the thermometer, when air is admitted into an exhausted receiver, is 50°. But as he does not inform us how far the exhaustion was carried in his experiments, we can draw no conclusions from his ingenious deductions. Professor Leslie, of Edinburgh, made a very ingenious set of experiments on this subject, which it will be proper to state.

Heat evolved when air is condensed.

He employed an excellent air-pump, and a glass receiver of the very largest dimensions, and approaching somewhat to the spherical form. A very delicate thermometer was suspended vertically a few inches above the plate of the air-pump. One-fifth of the air of the receiver was exhausted, and the apparatus left at liberty, till the enclosed thermometer had acquired the temperature of the room. The stop-cock was then suddenly opened, and the air allowed to return into the receiver. The thermometer rose very rapidly three centesimal degrees, from which point it afterwards slowly descended. Two-fifths of the air were then exhausted, and the experiment repeated. In like manner the rise of the thermometer was observed when three-fifths of the air, four-fifths, and $\frac{299}{300}$dths were exhausted and allowed to return suddenly into the receiver. The following table shows the results :—

Air withdrawn and then let in.	Rise of the thermometer.
$\frac{1}{5}$	3° centigrade
$\frac{2}{5}$	5·3
$\frac{3}{5}$	7
$\frac{4}{5}$	8
$\frac{299}{300}$	8·3

Before any conclusion could be drawn from these experiments, it was necessary to know what part of the heat was consumed on the sides of the receiver. To be able to form some idea of this, the experiments were repeated with another receiver of the same shape as the former, but of only half the size. The following table exhibits the results :—

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Rarefaction.		Rise of therm.
$\frac{1}{5}$	. . . .	1°·8 centigrade
$\frac{2}{5}$	. . . .	3·2
$\frac{3}{5}$	. . . .	4·2
$\frac{4}{5}$	. . . .	4·8
$\frac{999}{1000}$	. . .	5

These new quantities obviously follow the same progression : the former. They are in fact each $\frac{3}{5}$ths of the former number Now, the smaller receiver having under the fourth part of tl surface of the larger only the eighth part of its contents, e poses comparatively twice the extent of surface : hence tl rise of temperature which takes place in its included air mu be the same as would have happened, if, while its capacity r mained the same, the surface of the first receiver had bee exactly doubled.

Mr Leslie very ingeniously supposes that the air holds o part of the heat, while two parts and four parts are respe tively expended on the inside of the receivers. If we adm this conjecture, the results correspond accurately with obse vation ; for the air in the large receiver would retain on third, and in the small one-fifth of the whole heat evolved, a the rise of temperature in the two sets of experiments wou be as 5 to 3, as was actually the case. If we multiply the ri of the first thermometer by 3, we obtain the true quantity heat evolved, as in the following table :—

Rarefaction.		Rise of therm.
$\frac{1}{5}$	. . . .	9° centigrade
$\frac{2}{5}$	. . . .	16
$\frac{3}{5}$	. . . .	21
$\frac{4}{5}$	. . . .	24
$\frac{999}{1000}$	. . . .	25

But these numbers cannot indicate the heat evolved by storing the usual density to the whole air in the receiver. the first experiment, only $\frac{4}{5}$ths of the air in the receiver we restored to the usual density ; for the $\frac{1}{5}$th admitted had ready the density of the external air. In the second expe ment, $\frac{3}{5}$ths of the air were restored to their usual density ; the third $\frac{2}{5}$ths, in the fourth $\frac{1}{5}$th, and in the fifth $\frac{1}{1000}$dth. obtain the true numbers we must add $\frac{1}{5}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, and $\frac{999}{1000}$ su

cessively to each number. We obtain the true heat evolved as follows:— Sect. IV.

Rarefaction.		Rise of therm.
$\frac{1}{5}$	. .	11°·25 centigrade
$\frac{2}{5}$	. .	26·6
$\frac{3}{5}$	. .	52·5
$\frac{4}{5}$	. .	120
$\frac{999}{1000}$	. .	7500

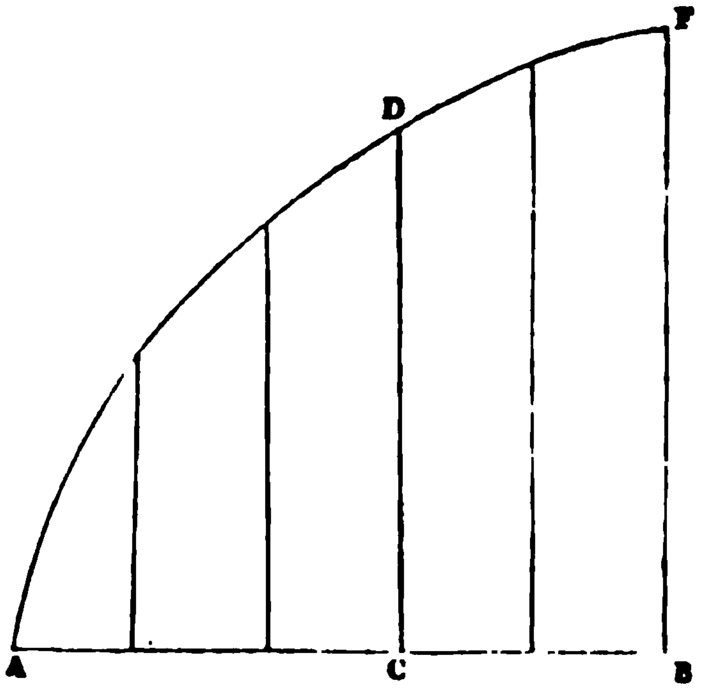
Thus according to the experiments and hypothesis of Leslie, when air of the following densities is restored to its original density represented by 1, the heat evolved is as follows:—

Density of the air.		Heat evolved.
1·	. .	0° Fahrenheit
0·8	. .	20·625
0·6	. .	48·
0·4	. .	94·5
0·2	. .	216
0·0003	. .	13500

Now, this evolution of heat is obviously owing entirely to a diminution of the specific heat of the air by condensation. It is obvious that there must be a corresponding increase of specific heat from rarefaction.

Let θ denote the density of the air, then $45\left(\frac{1}{\theta}-\theta\right)$ will denote the heat evolved, when the density is reduced to unity; *

* It will be proper to explain how this formula was obtained by Mr eslie.



Let A F be a portion of a para-
la, having B F for its axis, and
. B for an ordinate; and let A B
: B F. Divide the ordinate A B
to five equal parts, representing
e five equal divisions of the mer-
ial column corresponding to the
ferent densities of the air repre-
nted in the first column of the
le given in page 109. From
h of these divisions, raise per-
ndiculars C D, parallel to B F,

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or if the air be condensed, then $45\left(\theta-\frac{1}{\theta}\right)$ will denote the cold evolved, when the air is rarefied to 1. Let it be required to determine by this formula, how much heat is evolved

and meeting the curve. Then, if B C be the density of the remaining air, the rise of temperature is exhibited by C D. For, from the nature of the curve, (2 A B—A C) A C = C D × P, (the parameter, which is equal to B F). But A B was made = 5, and B F = 5°, the ultimate rise of the thermometer. Whence $C D = \frac{(10-A C) A C}{5}$. When the density is $\frac{2}{5}$, A C = 3, and C D becomes 4·2, as in the table (page 109). The conformity of the numbers in the table, with the abscisses of the parabola, is therefore evident.

It was remarked in the text, that the two tables exhibited in pages 109 and 110 are to each other as 5 to 3. In these two sets of experiments, the different sizes of the receivers only affect the experimental results by the different ratios between their surfaces and contents; since in other respects, as the proportion is the same, the temperature must be unaffected, the quantity of air and the heat developed being always in the same proportion. Let then x = number of additional degrees, to which the heat absorbed by the large glass would raise the temperature of the air, supposing the corresponding quantity of developed heat actually received by the air = 1. Then the temperature to which the included air would have risen, had the glass not affected it, is to the actual rise as $1 + x : 1$.

Again, since in the glass with the smallest diameter, the relative effect of it is twice as great as that of the glass with the large diameter; hence in it, if the air absorb 1, the glass will absorb $2x$ of heat. So that the total heat will be to the actual rise, as $1 + 2x : 1$. But in the two sets of experiments, the actual rise of the thermometer is as 3 : 5; 3, being the rise in the smaller, and 5, in the large receiver. Therefore, $3(1 + 2x)$ = heat actually evolved in the small receiver, and $5(1 + x)$ = heat actually evolved in the large; supposing the receivers not to have absorbed any. But the circumstances of the two experiments being absolutely the same, these two quantities must be equal. We have therefore $3(1 + 2x) = 5(1 + x)$; or $x = 2$. Hence the whole heat evolved in the small receiver, was five times the rise of the thermometer; and in the large, thrice that of the rise of the thermometer; or the true rise is indicated by the following table:—

$\frac{4}{5}$ . . .	9° centigrade
$\frac{3}{5}$ . . .	16
$\frac{2}{5}$ . . .	21
$\frac{1}{5}$ . . .	24
Extreme . .	25

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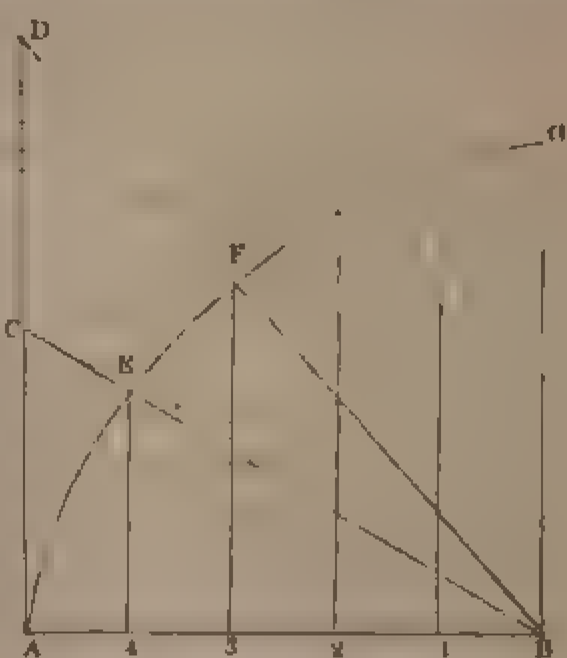
...n the density of air is doubled. Here $\delta = 2$. The for...la becomes 45 $(2 - \frac{1}{2})$ or 45 × 1·5=67°·5 Fahrenheit. ...quired the heat evolved, when air is condensed 30 times. ...re $\delta = 30$, we have 45 $(30 - \frac{1}{30}) = 45 \times 29{\cdot}966 = 1348°{\cdot}5$.

Let us apply this formula to the diminution of heat as we ...nd in the atmosphere. In an observation by Lasius, the ...ometer at Goslar stood at 29·5 inches, and at the top of



...et A G be part of a parabola ...efore, G B the absciss, and A B ... ordinate. Divide A B into ... number of equal parts, then if ... B be the density of the air, and ..., B 3, &c. any other densities, ... E 4, F 3, will represent the ...responding thermometrical rises ...formerly explained. But it must ... observed, that these rises are ... heats actually evolved, only ...en the air is suffered to regain ...usual density. And as the ca...city of air for heat is inversely ...its density, these results must be ...reased in the ratio of B 4, B 3, &c. to B A to give the true heats cor...ponding to these densities; or the heats belonging to the densities B 4, ..., &c. are respectively as A C, A D, &c. Now, by the nature of the ...rabola, A 4 (A B + B 4) or A B² — B 4² = P + E 4 · or E 4 ∝ (*i. e.* ...es as) A B² — B 4². But B 4 : B A : : E 4 : A C; or A C ∝ $\frac{E\,4}{B\,4}$; ... A C ∝ $\frac{A\,B^2 - B\,4^2}{B\,4}$. Now, if A B = 1, and B 4 = x, then A C ... $\frac{1-x^2}{x} \propto \left(\frac{1}{x} - x\right)$, or A C = $\left(\frac{1}{x} - x\right) y$ where y is a constant co-effi...t to be determined by experiment. Let us assume any of the densities ... in the above table, as $\frac{3}{5}$, the corresponding increment is 16, which, ...e the true increment, is to be increased in the ratio of $\frac{3}{5}$: 1 or 3 : 5, ...n $\frac{16 \times 5}{3} = \frac{80}{3}$. Now, in the formula, x is $\frac{3}{5}$, whence it becomes $\left(\frac{5}{3} - \frac{3}{5}\right) y = \frac{80}{3}$ or $\left(\frac{25-9}{5}\right) y = 80$. Hence, $y = 25$. Wherefore, the ...ement of temperature, at any density, x is 25° $\left(\frac{1}{x} - x\right)$, the density of ...mon air being 1. In the text, I have substituted 45 for 25, in order ...dapt the formula to Fahrenheit's scale.

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the Brocken, at 26·444 inches: required from this the difference of temperature. If we reckon the density of air at Goslar 1, to find its density at the top of the Brocken, we say 29·5 : 26·444 : : 1 : 0·896 = density of air at the top of the Brocken. By substitution, the formula becomes 45 (1·116—0·896) = 45 × 0·22 = 9°·9 equal to the difference between the air at Goslar and at the top of the Brocken. Lasius found the difference 9°·4, which differs only by half a degree from that given by the formula.

This formula, however, though it furnishes a valuable approximation, is not quite correct; for if we calculate from it what the mean temperature ought to be on the summit of Ben-Nevis, we obtain about 32°.* Yet we know that the summit of that mountain is below the level of perpetual congelation, which in Scotland is elevated to the height of about 6500 feet, exceeding that of any mountain in Great Britain.

It is obvious from the details here entered into, that the temperature of the air must diminish in proportion as its height increases above the level of the sea; because its density diminishes, and consequently its specific heat increases exactly in that ratio. But the same weight of air, at all elevations, above any place, contains exactly the same quantity of heat. So that if a quantity of air were suddenly transported from an elevated region to the level of the sea, its density would be continually increasing during its descent, while its specific heat would diminish in the same proportion, and when it reached the level of the sea, its temperature in consequence would be just as high as that of other portions of air in the same latitude and elevation. Air, therefore, does not feel cold

The same weight of air at all heights contains the same heat.

* This calculation is made on the supposition that the mean annual temperature at Fort-William is 47°. It lies about 53 minutes farther north than Glasgow. Now, the mean temperature at Glasgow is very nearly 47°·75; while the mean temperature of London is nearly 50°. The difference between these two temperatures is $2°\frac{1}{4}$, while the difference between the latitudes is very nearly 4 degrees. From this it would appear, that in Great Britain, every additional degree of latitude diminishes the temperature about 0·55 of a degree of Fahrenheit. It is obvious from this, that the mean heat at Fort-William is not under 47°. It should be 47°·3; which would make the mean heat at the summit of Ben-Nevis 32°·4.

in consequence of falling from an elevated situation; though this be an opinion commonly entertained; but in consequence of its being suddenly transported from a more northerly to a more southerly situation. Thus, the unequal distribution of temperature in the atmosphere, when properly understood, constitutes no objection to the mobility and ascent of air, when its temperature is increased.

SECTION V.—OF THE RADIATION, TRANSMISSION, AND POLARIZATION OF HEAT.

The radiation of heat having been first investigated by means of the thermometer, and the transmission and polarization of heat taking place when it passes in rays, the subject naturally claims a place in this chapter.

Experiments of Mariotte.

The first rude attempt to examine radiation was made by Mariotte. In the year 1682, he mentioned, at a meeting of the French academy of sciences, that the heat of a fire reflected by a burning mirror is sensible in its focus; but when a pane of glass is interposed between the mirror and its focus, the heat is no longer sensible.* Lambert, the celebrated German mathematician, was one of the first persons who repeated and varied this experiment. He placed burning charcoal in the focus of a mirror, while another mirror was standing parallel to it, at the distance of 24 feet. A combustible body placed in the focus of this last mirror was kindled. Yet when the light of a clear burning fire was collected in the focus of a large lens, the heat evolved was scarce sensible to the hand.

Of Lambert.

Of Scheele.

Scheele, in his *Treatise on Air and Fire*, published in 1777, made many important and new observations on the radiation of heat. He observed that the radiant heat from the fire passes through the air without heating that fluid, and that no aerial currents, however strong, have any tendency to intercept it. If we interpose a pane of glass between the fire and our face, the light passes through the glass with but little diminution; but the whole of the heat is intercepted. A glass mirror reflects the *light* of a fire, but not its *heat*, which it ab-

* Mem. Paris, i. 344.

sorbs and retains, while a polished metallic m
both the light and the heat. The glass, when
the fire, soon becomes hot, but a polished metal
a long time in that position without becoming se
But if we *blacken* its surface by holding it ov
candle, it cannot be kept four minutes in the hand
fire without acquiring so much heat as to excite

ie- ictet, ing. These experiments of Scheele were repeated a
Saussure, Pictet, and King, between the years 17
they were the first to introduce specula of tin,
derably facilitated the future investigation of the

ers- In the year 1800, an elaborate paper on the
solar rays, was published in the Philosophical
by Sir William Herschell. He had been occupi
telescopic observations on the sun, and employ
glasses to enable him to look at that luminary
that when they were dark enough to exclude
light, they soon became so hot as to crack and bre
This led him to investigate the heating powers of
coloured rays of the solar spectrum.

It is well known that when a solar ray is made to
a triangular prism of glass, and received upon
at some considerable distance, instead of a roun
sun, it forms an oblong spectrum consisting of
coloured rays; namely, *red, orange, yellow, gre*
and violet, of which the *red* is the least and th
refracted, and the refrangibility of the other
of their names. He found (as had been d
that the violet ray possesses the least heating
the heating power of the spectrum increase

Heating power of the rays. end to the red extremity of the spectrum
little table shows the rise of the thermomete
the green ray, and the red ray :—

Violet	.	16°
Green	.	22·4
Red	.	55

* Scheele on Air and Fire, p. 66. En

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Herschell had previously examined the illuminating power of the different coloured rays of the spectrum, and he found that the greatest illuminating power resided in the middle of the spectrum, that part of it in which the yellow and the green pass into each other. The violet ray has the least illuminating power; that of the red ray is greater, though much less than the yellow-green. The illuminating powers of these three rays he found as follows:— Their illuminating power.

Violet	. .	1
Red	. .	2
Yellow-green	.	4

This remarkable difference between the law which the rays of the solar spectrum follow in illuminating and heating bodies, led Herschell to suspect that in all probability the heating power does not terminate with the visible spectrum, but extends to some distance beyond it. Upon placing the thermometer below the limit of the solar spectrum, he found that it still continued to rise. The maximum point of elevation lay between $\frac{1}{2}$ and $\frac{1}{2}$ inch beyond the red ray, the spectrum being received on a board 52 inches distant from the prism. The heating power still continued sensible at the distance of $1\frac{1}{2}$ inch from the extremities of the red ray.

These experiments were repeated soon after by Mr Leslie, who employed for the purpose his differential thermometer. He found the greatest heat in the red ray, and no effect whatever beyond the visible spectrum. This induced him to call in question the accuracy of Herschell's observations, and to refuse his assent to them.* Landriani had already announced that the greatest heat of the prismatic spectrum was in the yellow ray;† and Rochon had found the point of greatest heat situated between the yellow and the red ray;‡ but the observations of these philosophers do not seem to have been known either to Herschell or Leslie. Experiments of Leslie.

Sir Henry Englefield repeated Herschell's experiments soon Of Englefield.

* Nicholson's 4to Journal, iv. 244.

† Volta; lettere sull'aria inflamabile nativa della paludi; 1777, p. 136.

‡ Recueil de Memoires, sur la Mecanique et la Physique, 1785, p. 348.

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after the appearance of Leslie's paper. His results agreed with those of Herschell. He found the greatest heat beyond the limit of the red ray.*

Of Berard.

They were repeated again, in 1813, by M. Berard, in Berthollet's laboratory, by means of an apparatus furnished at the expense of that eminent chemical philosopher. He employed a heliostade, by means of which the prismatic spectrum was rendered stationary. He found the heating power of the violet ray least, and it gradually augmented as the thermometer passed towards the red extremity of the spectrum. But the heating power did not terminate with the red ray; it was sensible half an inch beyond it, though at that distance it was diminished to one-fifth.†

These different experiments and observations appeared so contradictory, and so inconsistent with each other, that chemists were at a loss what conclusions to draw. The subject was partially elucidated by Wunch,‡ and still more completely by Seebeck,§ whose experiments appear to have been continued for at least ten years. Seebeck found, as all preceding observers had done, that the thermometer was elevated when placed in any one of the coloured rays of the solar spectrum. But the position of the point of greatest intensity depends upon *the nature of the prism* by which these rays are refracted.

Experiments of Seebeck.

When a hollow glass prism filled with *water*, or *oil of turpentine*, or *alcohol*, is employed, the point where the intensity of the heat is the greatest is situated in the *yellow* ray.

When the prism is filled with a solution of corrosive sublimate, and sal ammoniac, (recommended by Blair in his paper on the achromatic telescope,) or with sulphuric acid, the *greatest heat* is in the *orange* ray.

When the prism is of *crown glass* or common *white glass*, the greatest heat is in the *red* ray.

When flint-glass prisms are used, the heat is greatest with-

* Royal Institution Journ. 1802, p. 202.

† Ann. de Chim. lxxxv. 309.

‡ Magazin der Gesellsch. naturf. Freund. in Berlin, 1807.

§ Achandl. der Königk, Abad. Wissenschaften, in Berlin, 1818-19, p. 305.

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at the spectrum at some distance beyond the *red* ray. When Bohemian flint-glass prisms were employed, the greatest heat was still beyond the *red* ray; but nearer than when English flint-glass prisms were employed.

These facts being known and understood, it is easy to account for the different results obtained by different experimenters. They depended obviously upon the kind of prism employed by each.

Thus it is established that heating rays are found in every part of the coloured spectrum; but they are accumulated in greatest abundance at the least refracted end. When a flint glass prism is used, they are accumulated in greatest abundance beyond the red ray, and quite beyond the visible spectrum. Since the heating and illuminating rays may be thus separated, we have no evidence that they are the same. They seem rather to consist of two distinct kinds of rays, one of which illuminates, and the other heats objects.

Chemical rays most refrangible.

Besides these two species of rays, the solar spectrum appears to contain a third species of ray, which has been distinguished by the name of *chemical ray*, because it produces chemical changes on bodies. If a piece of chalk be immersed in a solution of *nitrate of silver*, and then exposed to the direct rays of the sun, it acquires a dark colour, owing to the deposition of a thin pellicle of silver upon its surface.* Chloride of silver is almost instantly blackened by exposure to the direct rays of the sun. In the same manner oxides of gold and mercury are reduced when exposed to the solar ray. Scheele, and after him Senebier, found that these changes are not produced by the red ray, nor by the rays near the least refrangible end of the spectrum. A feeble effect is produced by the green ray; but the greatest and most rapid alteration is produced when the oxides or chlorides are exposed to the violet or most refrangible ray.

When plants grow in the dark, they are said to be *etiolated*, and their colour is white. When such a plant is exposed to

* Mr Scanlan has shown, that nitrate of silver is not blackened by exposure to light, unless it is mixed with some organic matter. Thus it becomes black if allowed to touch paper.

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sunshine, it speedily begins to assume a green colour. Now, in this case also, the least refrangible rays have but little or no effect; but the change takes place rapidly, when the plant is placed in the *violet ray*. Berard found that when the least refrangible half of the solar spectrum is collected into a focus by a convex glass, it forms an exceedingly brilliant and colourless light, in which chloride of silver may be kept without undergoing any alteration in colour. But when it is put into the focus formed by concentrating the most refrangible half of the spectrum, though this focus is much less brilliant, it blackens in a few minutes. Dr Wollaston found that chloride of silver blackened still more speedily, than it did in the violet ray, when placed entirely above the visible spectrum at some distance from the limit of the violet ray. The same result was obtained nearly about the same time, by Messrs Ritter and Bockmann, in Germany. Thus it appears that the chemical rays are still more refrangible than the illuminating rays, and that they extend beyond them.*

Solar spectrum contains three kinds of rays.

The solar spectrum, then, consists of at least three distinct species of rays; namely, *illuminating rays*, constituting the visible spectrum: *heating rays*, less refrangible than the visible rays, and varying in the place of their greatest intensity according to the nature of the prism through which they are made to pass; and *chemical rays*, more refrangible than the others, and having the place of their greatest intensity beyond the uppermost limit of the visible spectrum. These three kinds of rays move in straight lines from the sun. We know not whether they all possess the same velocity; though there is no proof whatever of any difference in this respect.

Heat radiates from surfaces.

Rays of heat are capable of radiating from the surface of hot bodies, precisely as rays of light do from luminous bodies. The most important facts respecting the differences between bodies as radiators of heat, were ascertained by Mr Leslie, of Edinburgh, and made known by the publication of his *Treatise*

* Seebeck observed that chloride of silver when put into the red ray, acquires a pale rose colour. This is occasioned by the heat, and the effect accordingly is greatest in the hottest part of the spectrum.

on *Heat*, in 1804. It will be proper to give a view of the most important facts discovered by this very ingenious experimenter. Sect. V.

1. Mr Leslie filled with hot water a thin globe of bright tin, four inches in diameter, having a narrow neck, and placed it on a slender frame in a warm room without a fire. The thermometer inserted in this globe sunk half-way from the original temperature of the water to that of the room in 156 minutes. The same experiment was repeated, but the outside of the globe was now covered with a thin coat of lamp black. The time elapsed in cooling to the same temperature as in the last case was now only 81 minutes.* Here the rate of cooling was nearly doubled; yet the only difference was the thin covering of lamp black. Nothing can afford a more striking proof than this of the effect of the surface of the hot body on the rate of its cooling. Effect of surface on cooling.

Count Rumford took two thin cylindrical brass vessels of the same size and shape, filled them both with hot water of the same temperature, and clothed the one with a covering of Irish linen, but left the other naked. The naked vessel cooled ten degrees in 55 minutes, but the one covered with linen cooled ten degrees in 36½ minutes.† In this experiment, the linen produced a similar effect with the lamp black in the preceding. Instead of retarding the escape of heat, as might have been expected, it produced the contrary effect. The same acceleration took place when the cylinder was coated with a thin covering of glue, of black or white paint, or when it was smoked with a candle.

2. The variation in the rate of cooling occasioned by coating the hot vessel with different substances is greatest when the air of the room in which the experiments are made is perfectly still. The difference diminishes when the atmosphere is agitated, and in very strong winds it disappears almost entirely. Thus two globes of tin, one bright, the other covered with lamp black, being filled with hot water, and exposed to Greatest in still air.

* Leslie's Inquiry into the Nature of Heat, p. 268.

† Nicholson's Jour. ix. 60.

Chap. I. winds of various degrees of violence, were found by Mr Le[slie] to lose half their heat in the following times :—*

	Clean Globe.	Blackened Globe
In a gentle gale .	44′	35′
In a pretty strong breeze	23	20¼
In a vehement wind	9·5	9

This is sufficient to convince us, that the effect of the la[mp] black in accelerating cooling cannot be owing to any pow[er] which it has of *conducting* heat, and communicating it to t[he] air, but to the property which it has of radiating heat (to u[se] the common expression) in a greater degree than clear metal[lic] bodies.† That this is in reality the case is easily shown.

3. When a canister of tin, of a cubic shape and considerab[le] size, filled with hot water, is placed at the distance of a foot [or] two from a concave mirror of bright polished brass, having [a] delicate thermometer in the focus, the thermometer experienc[es] a certain elevation. If the canister be coated with la[mp] black, the thermometer rises much higher than when the met[al] is left bright. Here we perceive that more heat radiates fr[om] the lamp black than the clear metal; since the elevation of t[he] thermometer is in some degree the measure of the radiati[on]. A common thermometer does not answer well in similar exp[e]riments, because it is affected by every change of temperatu[re] in the room in which the experiments are made. But [Mr] Leslie has invented another, to which we are indebted for the precision that has been introduced into the subject. [He] has distinguished it by the name of the *differential thermomet[er]*. It was employed also by Count Rumford in his researches.

Differential thermometer.

This thermometer consists of a small glass tube bent i[nto] the shape of the letter U, and terminating at each extre[mity] in a small hollow ball, nearly of the same size; the tube c[on]tains a little sulphuric acid tinged red with carmine, and [suf]ficient to fill the greatest part of it. The glass balls are

* Inqury into the Nature of Heat, p. 271.

† Petit and Dulong have shown by direct experiments, that the qua[ntity] of heat taken away from hot bodies by gas by *conduction* is independe[nt of] the nature of the surface of the hot body. See Annals of Philoso[phy,] xiii. 322.

air, and both communicate with the intermediate tube. To of the legs of the tube is affixed a small ivory scale, divid- into 100 degrees; and the sulphuric acid is so disposed, t in the graduated leg its upper surface stands opposite to part of the scale marked 0. The glass ball attached to the of the instrument to which the scale is attached, is, by way distinction, called the *focal ball*. Suppose this thermometer ought into a warm room, the heat will act equally upon both lls, and, expanding the included air equally in each, the por in the tube will remain stationary. But, suppose the l ball exposed to heat while the other ball is not; in that e the air included in the focal ball will expand, while that the other is not affected. It will therefore press more upon e liquid in the tube, which will of course advance towards e cold ball, and therefore the liquid will rise in the tube ove 0, and the rise will be proportional to the degree of at applied to the focal ball. This thermometer, therefore, peculiarly adapted for ascertaining the degree of heat accu- lated in a particular point, while the surrounding atmo- here is but little affected, as happens in the focus of a reflect- g mirror. No change in the temperature of the room in ich the instrument is kept is indicated by it, while the ghtest alteration in the spot where the focal ball is placed is mediately announced by it.

In making experiments on the radiation of heat, Mr Leslie ployed hollow tin cubes, varying in size from three inches ten, filled with hot water, and placed before a tin reflec- , having the differential thermometer in the focus. The flector employed was of the parabolic figure, and about 14 ches in diameter. This apparatus afforded the means of as- rtaining the effect of different surfaces in radiating heat. was only necessary to coat the surface of the canister th the various substances whose radiating properties were be tried, and expose it, thus coated and filled with hot water, fore the reflector. The heat radiated in each case would collected into the focus where the focal ball of the differen- l thermometer was placed, and the rise of this instrument ould indicate the proportional radiation of each surface.

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These experiments were conducted with much address. The following are the principal results obtained:—

Radiation as the excess of heat above the air.

4. When the nature and position of the canister is the same, the rise of the differential thermometer is always proportional to the difference between the temperature of the hot canister and that of the air in the room in which the experiment is made.*

Rise of thermometer inversely as the distance.

5. When the temperature of the canister is the same, the effect upon the differential thermometer diminishes as the distance of the canister increases from the reflector, the focal ball being always understood to be placed in the focus of the mirror. Thus, if the rise of the thermometer, when the canister was three feet from the mirror, be denoted by 100, it will amount only to 57 when the canister is removed to six feet. On substituting a glass mirror for the reflector, and a charcoal fire for the canister, when the fire was at the distance of 10 feet the thermometer rose 37°, and at the distance of 30 feet it rose 21°.† From Mr Leslie's experiments it follows, that the effect on the thermometer is very nearly inversely proportional to the distance of the canister from the reflector. He found likewise that when canisters of different sizes were used, heated to the same point, and placed at such distances that they all subtended the same angle at the reflector; in that case the effect of each upon the differential thermometer was nearly the same. Thus a canister of

3 inches at	3 feet distance raised the thermometer .	50°
4 ———	4	54
6 ———	6	57
10 ———	10	59

From these experiments we learn, that the effect of the canister upon the thermometer is nearly proportional to the angle which it subtends, and likewise that the heat radiated from the canister suffers no sensible diminution during its passage through the air.

Heat radiates in all directions.

6. Heat radiates from the surface of hot bodies in all directions; but from Mr Leslie's experiments we learn, that the

* Leslie, p. 14. † Ibid. p. 51.

radiation is most copious in the direction perpendicular to the surface of the hot body. When the canister is placed in an oblique position to the reflector, the effect diminishes, and the diminution increases with the obliquity of the canister. Mr Leslie has shown, that the effect in all positions is proportional to the visual magnitude of the canister as seen from the reflector, or to its orthographic projection. Hence the action of the heated surface is proportional to the sine of its inclination to the reflector.

Such are the effects of the temperature, the distance, and position of the canister with respect to the reflector. None of these, except the first, occasion any variation in the quantity of heat radiated, but merely in that portion of it which is collected by the mirror and sent to the focal ball; but the case is different when the surface of the canister itself is altered.

7. Mr Leslie ascertained the power of different substances to radiate, by applying them in succession to a side of the canister, and observing what effect was produced upon the differential thermometer. The following table exhibits the relative power of the different substances tried by that philosopher, expressed by the elevation of the differential thermometer produced:—

Radiating power of different bodies.

Lamp black	100	Isinglass	80
Water by estimate	100+	Plumbago	75
Writing paper	98	Tarnished lead	45
Rosin	96	Mercury	20+
Sealing wax	95	Clean lead	19
Crown glass	90	Iron polished	15
China ink	88	Tin plate	12
Ice	85	Gold, silver, copper	12
Minium	80		

From this table it appears, that the metals radiate much worse than other substances, and that tin plate is one of the feeblest of the metallic bodies tried. Lamp black radiates more than eight times as much as this last metal, and crown glass 7·5 times as much.

8. Such are the radiating powers of different substances. But even when the substance continues the same, the radiation very considerably modified by apparently trifling alterations

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on its surface. Thus metals radiate more imperfectly than other bodies; but this imperfection depends upon the brightness and smoothness of their surface. When, by exposure to the air, the metal acquires that tarnish which is usually ascribed at present to oxidizement, the power of radiating heat is greatly increased. Thus it appears from the preceding table, that the radiating power of lead, while bright, is only 19; but when its surface becomes tarnished, its radiating power becomes no less than 45. The same change happens to tin, and to all the metals tried.

Increased by tarnishing,

When the smoothness of the surface is destroyed by scratching the metal, its radiating power is increased. Thus, if the effect of a bright side of the canister be 12, it will be raised to 22 by rubbing the side in one direction with a bit of fine sand paper.* But when the surface is rubbed across with sand paper, so as to form a new set of furrows intersecting the former ones, the radiating power is again somewhat diminished.

and scratching the surface.

9. The radiating power of the different substances examined was ascertained by applying a thin covering of each to one of the sides of the canister. Now this coat may vary in thickness in any given degree. It becomes a question of some importance to ascertain, whether the radiating power is influenced by the thickness to a given extent, or whether it continues the same, whatever be the thickness of the covering coat. This question Mr Leslie has likewise resolved. On a bright side of a canister he spread a thin coat of liquefied jelly, and four times the quantity upon another side; both dried into very thin films. The effect of the thinnest film was 38, that of the other 54. In this case the effect increased with the thickness of the coat. The augmentation goes on till the thickness of the coat of jelly amounts to about $\frac{1}{1000}$dth of an inch; after which it remains stationary. When a surface of the canister was rubbed with olive oil, the effect was 51: a thicker coat of oil produced an effect of 59. Thus it appears that when a metallic surface is covered with a coat of jelly or oil, the effect is proportional to the thickness of the coat, till this thickness

Varies with the thickness of the coating.

* Leslie, p. 81

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amounts to a certain quantity; but when a vitreous surface is covered by very thin coats of metal, no such change is perceived. A canister was employed, one of the sides of which was a glass plate. Upon this plate were applied in succession, very fine coats of gold, silver, and copper leaf. But notwithstanding their thinness, the effect was only 12, or the same that would have been produced by a thick coat of these very metals. But when glass, enamelled with gold, is used, the effect is somewhat increased; a proof that varying the thickness of the metallic coats would have the same effect as varying the thickness of jelly, provided they could be procured of sufficient tenuity.* As long as an increase of thickness alters the radiating power of the coat, it is obvious that the surface of the canister below exerts a certain degree of energy; and the action exerted by metallic bodies appears to be greater than that exerted by vitreous bodies.

It was shown long ago by Dr Franklin, that the colour of bodies has a considerable effect upon the rate at which they absorb heat. He placed different coloured pieces of cloth, black, deep blue, lighter blue, green, purple, red, yellow, and white, upon snow, and exposed them to the direct rays of the sun. The pieces sank deeper in the snow in proportion as they absorbed more heat. Now, the black had sunk deepest, while the white had not sunk at all, and the others sank deeper in the order in which they are named, those nearest the black always sinking deepest. Sir Humphrey Davy made a similar experiment, about the commencement of his chemical career. Six pieces of copper, coloured black, blue, green, red, yellow, and white, with a small portion of cerate on their under surface, were exposed on a white board to the rays of the sun, and the time of the cerate melting noted. It took place in the order in which the colours have been named. More lately, Dr Stark has repeated these experiments in a different manner, and has shown that colour has a considerable effect both on the rate at which heat is absorbed and emitted.†

* Leslie, p. 110. † Phil. Trans. 1834, p. 287.

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He surrounded the bulb of a thermometer plunged into a glass tube with equal weights of wool dyed black, green, scarlet, and white. The tube was then plunged into boiling water, and the time which the thermometer took to rise from 50° to 170° was noted. It was when the bulb was surrounded by the different coloured wools as follows:—

Black	. .	4′ 30″
Dark green	.	5
Scarlet	. .	5 30
White	. .	8

He next coated the bulb of an air thermometer with black, dark brown, orange red, yellow, and white,* and the heat of an Argand lamp about 3 inches distance was thrown upon this bulb by means of a planished tin reflector. The rise with the different colours was as follows:—

Black	. .	83°
Brown	. .	74
Orange red	. .	58
Yellow	. .	53
White	. .	45

Dr Stark showed also that the rate at which these different coloured bodies cool, follows precisely the same law as that in which they acquire heat. He surrounded the bulb of the thermometer with different coloured wool, and noted the time that it took to sink from 170° to 50°. It was as follows:—

Black wool	. .	21′
Red	. . .	26
White	. . .	27

He surrounded the bulb with wheat flour coloured black, brown, yellow, and white;† and noted the time of cooling from 180° to 50°, as follows:—

Black flour	. .	9′ 50″
Brown	. .	11
Yellow	. .	12
White	. .	12 15

* The black was given by lamp black, the other colours by peroxide of lead, red lead, protoxide of lead, and carbonate of lead.

† Black by lamp black, brown by amber, yellow by gamboge.

From these experiments we see the reason why the skin of a negro is better suited to the torrid zone than that of a European. Sect. V.

10. When the focal ball is in its natural state, that is to say, when its surface is vitreous, it has been already observed, that the side of the hot canister coated with lamp black raises the thermometer 100°. If the experiment be repeated, covering the focal ball with a smooth surface of tinfoil, instead of rising to 100°, the thermometer will only indicate 20°. A bright side of the canister will raise the thermometer, when the focal ball is naked, 12°; but when the ball is covered with tinfoil, the elevation will not exceed $2\frac{1}{2}$°* From these experiments it is obvious, that metal not only radiates heat worse than glass, but likewise that it is not nearly so capable of imbibing it when the rays strike against its surface. If the surface of the tinfoil be furrowed by rubbing it with sand paper, the effect produced, when the focal ball is exposed in the focus, will be considerably increased.† It has been already observed that the radiating power of tin is likewise increased by scratching it. These facts entitle us to conclude, that those surfaces which radiate heat most powerfully, likewise absorb it most abundantly when it impinges against them. Surfaces radiate and absorb heat equally.

11. The very contrary holds with respect to the reflectors, as might indeed have been expected. Those surfaces which *radiate* heat best, *reflect* it worst; while the weakest *radiating surfaces* are the most *powerful* reflectors. Metals, of course, are much better reflectors than glass. When a glass mirror was used instead of the tin reflector, the differential thermometer rose only one degree; upon coating the surface of the mirror with lamp black, all effect was destroyed; when covered with a sheet of tinfoil the effect was 10°.‡ Reflection inversely as radiation.

To compare the relative intensity of different substances as reflectors, Mr Leslie placed thin smooth plates of the substances to be tried before the principal reflector, and nearer than the proper focus. A new reflection was produced, and the rays were collected in a focus as much nearer the reflec-

* Leslie, p. 19. † Ibid. p. 81. ‡ Ibid. p. 20.

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Reflecting power of various bodies.

tor than the plate as the old focus was farther distant. comparative power of the different substances tried w follows :—*

Brass . . .	100	Lead	
Silver . . .	90	Tinfoil softened by mercu	
Tinfoil . . .	85	Glass	
Block tin . . .	80	Ditto coated with wax or	
Steel . . .	70		

When the polish of the reflector is destroyed by rubbi with sand paper, the effect is very much diminished. the reflector is coated over with a solution of jelly, the is diminished in proportion as the thickness of the coa creases, till its diameter amounts to $\frac{1}{1000}$th part of an The following table exhibits the intensity of the refl coated with jelly of various degrees of thickness :—†

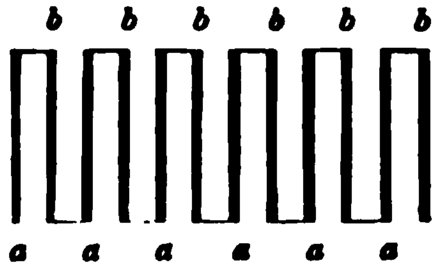
Thickness of coat.	Effect.
Naked Reflector . .	127
$\frac{1}{200000}$	98
$\frac{1}{100000}$	93
$\frac{1}{50000}$	87
$\frac{1}{20000}$	61
$\frac{1}{10000}$	39
$\frac{1}{5000}$	29
$\frac{1}{2000}$	21
$\frac{1}{1000}$	15

All these phenomena are precisely what might have expected, on the supposition that the intensity of reflecti inversely that of radiation. Mr Leslie has shown that the anterior surface of reflectors only that acts. For wl glass mirror is employed, its power is not altered by scra off the tin from its back, nor by grinding the posterior su with sand or emery.‡

Such was the state of our knowledge of the radiati heat, when Becquerel suggested, in the year 1826, a me measuring high degrees of heat, by means of the the electric currents.§ He formed his electric thermomete

* Leslie, p. 98. † Ibid. p. 106. ‡ Ibid. p. 21.
§ Ann. de Chim. et de Phys. xxxi. 371.

means of wires of platinum and palladium communicating with a galvanometer.* Some years afterwards, M. Nobili thought of constructing an electric thermoscope of great sensibility. He employed for the purpose, twelve small bars of bismuth and antimony, which act most powerfully in producing thermo-electric currents. They were soldered together at the extremity, and insulated everywhere else in the way represented in the diagram, where *a* represents the antimony bars, and *b* those of bismuth. They were packed in a box, so that only the two ends of the pile, where the metals were soldered together, were exposed. Copper



wires, attached to the extreme bar, (the one of bismuth, the other of antimony,) served to communicate with the galvanometer. He held the box in his hand, and touched with the exposed face of the pile the body whose temperature was to be determined. The diameter of the box was about two or three inches, and the base of each bar of metal was about one and a half inch square.

M. Melloni having ascertained by some trials, that the action of the thermo-electric pile or the *thermo-multiplier*, as it is called, depends more upon the numbers than the size of the elements of which it is composed, reduced each metallic bar to about one-thirtieth or one-fortieth of the size of those used by Nobili, and kept them isolated from each other, except where soldered, by slips of paper. He increased their numbers to thirty or forty pairs, and fixed them in a compact bundle, so that the two extremities, where the bars are soldered, were free. Several additions were made by him to this apparatus, which it is unnecessary to describe minutely in this place, as this has been already done by Nobili and Melloni in 1831.†

By means of this apparatus, which is capable of detecting

* Galvanometers, as will be explained in a subsequent part of this volume, consist of a magnetic needle suspended freely, and enclosed in numerous coils of insulated wire, through which the electricity that moves the needle passes.

† Ann. de Chim. et de Phys. xlviii. 198.

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much smaller quantities of heat than the differential thermometer,* and which acts with very great rapidity; a great number of interesting experiments were made, and a great variety of most important facts ascertained by Melloni,† and by Professor Forbes‡ of Edinburgh. I shall endeavour to lay the most striking of these new facts before the reader.

Heat radiates through solids.

12. Contrary to the opinion of Professor Leslie, heat is capable of radiating through many solid and liquid bodies, as well as through air and elastic fluids.

13. The bodies through which heat is transmitted in rays are generally transparent or translucent. The only exceptions which Melloni has met with, are black mica and black glass, both of which, though opaque, transmit heat from a lamp, from incandescent platinum, or from copper heated to 734°; but not from copper heated to 212°.

14. The quantity of heat transmitted through bodies is not proportional to their transparency, but (except in crystals) seems connected with their refrangibility. Liquids are in general less permeable to heat than solids. To bodies which transmit heat well, Melloni has given the name of *diathermanous* or *transcaloric* bodies.

Modified by thickness.

15. The quantity of heat transmitted through transparent glass, is modified by the thickness of the glass. This will appear from the following table, exhibiting the quantity of heat transmitted and absorbed by glasses of various thickness, as determined by Melloni,§ the whole heat being 1000:—

Thickness. Inches	Transmitted.	Absorbed.
0·0804	619	381
0·1624	576	424
0·2441	558	442
0·3256	549	451

We see from this that the greatest part of the heat

* From the experiments of Professor Forbes, it would appear that the deviation 1° of the thermo-multiplier, is equivalent to $\frac{1}{50}$th or $\frac{1}{60}$th of a centigrade degree.

† Ann. de Chim. et de Phys. liii. 5; lv. 337; and lxi. 5.

‡ Philosophical Magazine, (3d series), vi. 134, 205, 284, 366; vii. 349; xii. 545; xiii. 97, 180.

§ Ann. de Chim. et de Phys. liii. 39.

absorbed, is absorbed by the first portion of the plate of glass. Sect. V.
Suppose the thickness of glass to be divided in idea into four slices of equal thickness, the quantity of heat lost in passing through each would be 381, 43, 18, and 9.

It is evident from this, that the transmission of heat through glass follows a different law from that of light.

This is still farther confirmed by the following experiments of the same philosopher:—

Thickness of glass. Inches.	Rays transmitted.	Rays stopped.
1·063	484	516
2·126	380	620
3·189	303	697

Here the rate of stoppage is 516, 215, 203.

The following table from the same experimenter, shows the heat transmitted and absorbed in passing through various thicknesses of oil of colza:—

Thickness. Inches.	Rays transmitted.	Rays stopped.
0·2664	443	557
0·5328	363	637
1·0656	294	706
2·1312	270	730
2·8035	255	745
4·2630	244	756

If we suppose the thickest portion of the oil divided into six slices, the respective thicknesses of which are 0·2664 inch, 0·2664, 0·5328, 1·0656, 1·0656, 1·0656, we have the portions stopped at each slice as follows:—

1st Slice	0·557
2	0·180
3	0·190
4	0·082
5	0·056
6	0·040

Thus we see that the stoppage of heat in passing through slices of oil of colza, diminishes just as in glass.

It is obvious from this, that if a ray of heat be transmitted through a second plate of glass, much less heat will be stopped than was by the first plate.

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16. It is well known that the quantity of light which passes through diaphanous bodies, depends upon their transparency. If a body be perfectly transparent, the whole light will pass through.* If it be opaque, none will be transmitted; and the quantity through intermediate bodies will be proportioned to their degree of transparency. The same differences exists with regard to the transmission of heat through bodies. There is one body, *sal gemme*, (or native transparent common salt,) through which the whole heat passes, except what is reflected from its two surfaces. Suppose the whole heat to be 1000, Melloni found 923 of this, from whatever source, and though greatly below the point of being luminous, pass through a plate of sal gemme. The 77 lost are reflected from the surfaces of the salt. Melloni has shown that the quantity reflected by the anterior surface is 39·3; consequently the quantity reflected from the posterior surface must be 37·7.

Sal gemme transmits all the heat.

Thus, sal gemme, with respect to heat, is what a perfectly transparent body is to light. All other bodies, even though colourless and transparent, exert an action similar to that of coloured glass upon light; that is to say, they stop certain rays, and transmit others. The following table shows the quantity of heat transmitted through glasses of various colours, and 0·0728 inch in thickness.† The whole heat being 100.

Other bodies only a part of it.

	Heat transmitted.
Colourless glass	40
Deep red glass	33
Orange do.	29
Brilliant yellow	22
Apple green glass	25
Mineral green do.	23
Blue do.	21
Indigo,	12
Deep violet	34
Black and opaque	17

The effect of different bodies in stopping heat transmitted, is well exemplified in the following table of Melloni. The thickness of each body interposed as a screen, is 0·10236

* The portion reflected at the surface of the body always excepted.

† Ann. de Chim. et de Phys. lv. 379.

ı. Four sources of heat were employed; 1. A common p, called by Melloni, lamp of Locatelli; 2. A coil of plaım wire kept incandescent by vapour of alcohol; 3. Copper ckened by the smoke of a candle, and heated to 734°; Blackened copper plate heated to 212°. The two first ırces were luminous, the two last contained no sensible ıntity of light whatever:—* Sect. V.

Screens.	Heat transmitted. Total being 100°.			
	Lamp of Locatelli.	Incandescent platinum.	Copper heated to 734°.	Ditto heated to 212°.
ıl gemme	92	92	92	92
'luor spar, diaphanous and colourless	78	69	42	33
ıl gemme, diaphanous but muddy .	65	65	65	65
leryl, diaphanous, yellowish green .	54	23	13	0
'luor spar, diaphanous, greenish .	46	38	24	20
celand spar, diaphanous, colourless .	39	28	6	0
Do. do. do. .	38	28	5	0
rystal glass, diaphanous, colourless .	39	24	6	0
Do. do. do. .	38	26	5	0
lock crystal, diaphanous, colourless .	38	28	6	0
Do. smoked, diaphanous, brown .	37	28	6	0
ichromate of potash, diaphanous, orange	34	28	15	0
'opaz, diaphanous, colourless . .	33	24	4	0
arbonate of lead, diaphanous, colourless	32	23	4	0
ılphate of barytes, diaphanous, slightly muddy	24	18	3	0
hite agate, translucent, pearly .	23	11	2	0
lularia, diaphanous, muddy, veined	23	19	6	0
nethyst, diaphanous, violet . .	21	9	2	0
tificial amber, diaphanous, yellow .	21	5	0	0
ryl, diaphanous, bluish green .	19	13	2	0
ate, translucent, yellow . .	19	12	2	0
'ax, translucent, white . .	18	12	8	0
ırmalin, diaphanous, deep green .	18	16	3	0
ıorn, translucent, brown . .	18	4	0	0
ı, diaphanous, yellowish . .	18	3	0	0
hate of barytes, diaphanous and muddy, veined	17	11	3	0
hate of lime, diaphanous, colourless	14	5	0	0
ıelian, translucent, brown . .	14	7	2	0
ic acid, diaphanous, colourless .	11	2	0	0
ıonate of ammonia, diaphanous, muddy	12	3	0	0
ıelle salt, diaphanous, colourless .	11	3	0	0

* Ann. de Chim. et de Phys. lv. 347.

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Screens.	Heat transmitted. Total 817.° Lamp of Locatelli.	Incandescent platinum.	Copper heated to 734°.
Amber, translucent, yellowish . .	11	5	0
Alum, diaphanous, colourless . .	9	2	0
Glue, diaphanous, yellowish brown .	9	2	0
Mother of pearl, translucent, white .	9	0	0
Sugar candy, diaphanous, colourless .	8	0	0
Fluor spar, translucent, green .	8	6	4
Fused sugar, diaphanous, yellowish .	7	0	0
Ice, diaphanous, colourless . .	6	0	0

It is obvious from this table, that the quantity of transmitted bears no relation to the transparency of the b through which it passes; the quantity transmitted also dently increases in every body, (sal gemme excepted) a temperature of the source of heat increases.

The same principle was observed in all the liquids tri M. Melloni. The quantity of heat transmitted diminish the source of heat diminished, precisely as when the hea transmitted through solids. Out of twenty-eight liquids only three exceptions occurred; namely, bisulphuret of ca chloride of sulphur, and protochloride of phosphorus. Melloni considers these exceptions as only apparent, an real.

Thus it appears that radiant heat from different sourc absorbed in greater or smaller proportions in traversing phanous bodies, both solid and liquid. But for the same the absorption increases constantly as the temperature o source diminishes.

The case is different with light. The palest and feeble light, when passed through a transparent body, s just the same absorption as the strongest and brightest Sal gemme allows the same proportion of heat of wh intensity to pass through it. Heat, then, with respect gemme, is in the same predicament as light with resp transparent bodies. All the others intercept more and heat as the temperature of the source diminishes. It is from this, that these bodies allow certain rays of heat t

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while they intercept others. They act upon heat precisely as coloured bodies do upon light.

It follows from this that the rays of heat from different sources are not identic in their nature. The heat of boiling water, copper heated to 734°, incandescent platinum wire, and a flaming lamp, give each a greater quantity of rays of heat of a certain kind; just as coloured bodies give out each more light of a peculiar colour.

Bodies may be divided into *diathermic* and *athermic*. The diathermic are divisible into two classes, *universal* and *partial*. To the first of these, analogous to colourless transparent bodies, belongs sal gemme; to the second, corresponding to coloured bodies, belong all the bodies in the preceding table, together with liquids and diaphanous bodies in general.

The athermic bodies include all the opaque bodies hitherto examined, excepting *black glass* and *black mica*, which, though impervious to light, yet allow heat from certain sources to be transmitted. The following little table exhibits the quantity transmitted from the same four sources, according to the experiments of Melloni:—*

Screens.	Lamp Locatelli.	Incandescent platinum.	Copper of 734°.	Copper of 212°.
Black glass, thickness 0·03937 inch	26	25	12	0
Ditto thickness 0·07874 inch	16	15·5	8	0
Black mica, thickness 0·0236 inch	29	28	13	0
Ditto thickness 0·03543 inch	20	20	9	0

The whole heat is supposed as before to be 100.

These bodies, though opaque, are diathermic, but partially so, since they transmit certain rays of heat and intercept others.

Thus various bodies exercise the same power over heat that coloured bodies do over light; that is to say, they intercept certain rays while they transmit others. It is clear from the experiments of Herschell, that these rays differ in their refran-

* Ann de Chim. et de Phys. lv. 358.

Chap. I. gibility. It is easy from this to understand the reason why, in the solar spectrum, the point of greatest heat varies according to the nature of the prism used. Oil of turpentine stops a greater portion of the heat that would be transmitted in the orange and red rays, or below them, while it transmits that which passes with the yellow ray. Hence the point of greatest heat is in the yellow ray. A similar explanation accounts for the maximum heat being in the *orange* ray when the prism is a solution of corrosive sublimate or sal ammoniac; for its being in the red ray with a prism of crown glass, and below the red ray with a prism of flint glass.

New property of the solar spectrum.

17. It may be proper here to describe a new property of the solar spectrum, discovered by Melloni.* In a prism of crown glass, the maximum heat, as has been already observed, is in the red ray of the spectrum. The heat diminishes as we ascend, and is least of all in the violet ray. It diminishes also as we descend, and disappears at a certain distance below the red ray. In the lower part of the *obscure* spectrum (as it may be termed) lines may be drawn, indicating points where the heat is equal with that of the coloured rays above the maximum point in the red ray.

Now if we determine the heat of the violet ray, and then make it to pass through a layer of water, 1 line in thickness, the heat passes through the water with little diminution. If we try the line isothermal with the violet ray, below the red ray, we shall find that the whole is stopped by the water. As we *descend* from the violet ray, more and more of the heat is stopped by the water: but as we *ascend* from the line isothermal with the violet ray, below the red ray, more and more of the heat passes through the water. The following table gives the temperatures in the respective rays before and after the interposition of the water, in centigrade degrees:—

* Ann. de Chim. et de Phys. xlviii. 385.

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	Before.	After.	
Violet	2°	2	0·00
Indigo	5	4·5	0·10
Blue	9	8	0·11
Green	12	10	0·17
Yellow	25	20	0·28
Orange	29	21	0·42
Red	32	20	0·57
1st obscure band . .	29	14	0·70
2d do. do. . .	25	9	0·74
3d do. do. . .	12	3	0·75
4th do. do. . .	9	1	0·88
5th do. do. . .	5	0·5	0·90
6th do. do. . .	2	0	1·00

The last column gives the loss in hundred parts of the initial temperature, as calculated by Melloni.

18. Melloni, and Professor Forbes, have shown by decisive experiments, that radiant heat is refrangible as well as radiant light; and that its rays differ from each other in their refrangibility, for the refrangibility of heat from different sources was different. The rays from the lamp were more refrangible than those from incandescent platinum; and these last were more refrangible than the rays from copper heated to 734°.*

19. In the earlier experiments of Melloni, he did not find that the rays of heat were polarized when passed through the tourmalin. But he afterwards found that this conclusion was hasty, and that the tourmalin polarizes heat as well as light. The truth of this statement is shown very clearly by Professor Forbes.† They also polarized heat by plates of mica, and also by reflection, and they have shown that the polarization of light and heat follow exactly the same law. Heat polarized.

20. Professor Forbes has shown that the action of crystallized bodies on heat is precisely the same as on light, and that heat is capable of being refracted doubly by certain crystals as well as light, and that by them it is dipolarized as is the case with light. He has shown also that the velocities of the ordi- Doubly refracted.

* Ann. de Chim. et de Phys. lv. 367.
† Phil. Mag. (3d series) vi. 205, and xii. 549.

Chap. I. nary and extraordinary ray, in doubly refracting crystals, are totally different from those of light.*

21. Professor Forbes has made an interesting set of experiments on the refrangibility of heat, and has shown that it is less refrangible than light.† This indeed was sufficiently obvious from the old experiments of Herschell. But as heat is found in all the coloured rays of the spectrum, I do not see how it can be denied that some of its rays are as refrangible as the violet ray with which they are mixed.

22. From the facts stated in this section, it appears that heat is propagated, reflected, refracted, and polarized precisely as light is. Some mediums, as air and sal gemme, transmit equally all sorts of calorific and luminous rays; but others act differently on the two agents; sometimes absorbing more light than heat, and sometimes more heat than light. Some absorb the whole luminous rays while they are permeable to certain calorific rays, while others absorb the whole calorific rays while they are permeable to light.

Analogous differences are produced in the diffuse reflection which the two kinds of rays experience at the surface of opaque and athermic bodies. For we see perfectly white bodies reflect or absorb very different proportions of heat, according to the quality of the calorific rays; although these white surfaces absorb all the rays of light in equal proportions.

Other inequalities depending always on absorption, are evident in the phenomena of polarization, exhibited by the tourmalin. Here the two bundles into which a ray of light divides itself, on penetrating into the interior of the slices of tourmalin, are so modified in their progressive motions that the ordinary ray is entirely absorbed during its passage, and that the extraordinary ray alone presents itself at the emergence completely polarized. And this happens whatever be the colour of the incident light. The same thing does not take place with the rays of heat; the two bundles of which, produced at the entry into the polarizing plates, experience absorptions sometimes very different, and sometimes perfectly

* Phil. Mag. (3d series) xiii. 106, 192. † Ibid. xiii. 180.

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equal, occasioning great differences in the polarizations, according to the quality of the calorific rays.

The polarization becomes equal for all kinds of rays if we produce it by refraction and reflection, which are quite independent of the absorption of the mediums.

All bodies exposed to radiant heat, become hot; and when removed from the action of the rays preserve, for some time, the heat acquired. But very few substances, after being exposed to light, retain it so as to appear luminous in the dark.

Heat thus absorbed seems, so to speak, to have changed its nature. It forms a homogeneous current, and the mode of its transmission assumes characters quite opposite to that of radiant heat or light. It diffuses itself through the body in every direction, precisely as the heat communicated by contact, and this propagation is considerably modified by the displacement of the different parts of which the body is composed. Light and radiant heat, on the contrary, move only in straight lines, and traverse space with incredible velocity, without being influenced by the most violent agitation of the medium through which they pass.

The laws of these two great agents of nature, and the modifications which they experience by the action of ponderable matter, is the same as long as the rays move freely. But numerous differences appear as soon as the motions of the two sets of rays suffer an interception either at the surface or in the interior of bodies.

23. Sir John Leslie accounted for the radiation of heat by supposing that the source of heat occasions undulations or waves in the air, and that heat was propagated through air with the same velocity as sound. But the facts ascertained by Melloni and Forbes, are quite incompatible with such an explanation. The explanation which has been generally adopted, was originally started by M. Prevost, of Geneva, as early as 1791, and was afterwards expounded in a work entitled *Du Calorique Rayonnant*, published in the year 1809.

Prevost supposes that heat is a discrete fluid, every particle of which moves in a straight line. These particles go one in one direction, and another in another, so that every

Prevost's theory of radiation.

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sensible point of the hot space is a centre, from which depart, and to which arrive, rows of particles or calorific rays. The consequence of this must be, that if we suppose two neighbouring spaces, in which heat abounds, there must be a constant interchange of heat between them. If it abounds equally in each, the interchanges will balance each other, and the temperature will continue the same. If one contains more than the other, the interchanges will be unequal, and by a continued repetition of these unequal changes, the temperatures of both will be in time reduced to equality. If we suppose a body placed in a medium hotter than itself, and the temperature of that medium constant, we may consider the heat of the medium as consisting of two parts, one equal to that of the body, the other equal to the difference between the temperature of the two. The first of these two parts may be left out of view, being counterbalanced by the radiation of the body; the excess alone requires consideration, and relatively to that excess, the body is absolutely cold.

According to this theory, all bodies are constantly emitting rays of heat in all directions, and constantly receiving rays of heat from all directions. When the rays emitted and the rays received are equal in number, the temperature remains constant. When a body sends out more rays than it receives, its temperature sinks; when it receives more than it sends out, its temperature rises. If a body be placed in the neighbourhood of another, it will intercept a great many rays that were moving towards the second body, in the direction now blocked up. But if the temperature of the intercepting body be the same as that of those whose rays it intercepts, it will send out as many rays as it stops, and thus the temperature of the second body will not be affected. If the intercepting body be *colder* than those whose rays it intercepts, it will receive more than it can give out; the consequence of which will be, that the temperature of the second body will be reduced. The very contrary must happen if the intercepting body be hotter than those whose rays it intercepts.

Apparent radiation of cold explained.

This theory will enable us to explain a set of phenomena, which have occasioned much discussion, and been considered

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ry some as incompatible with the received notions concerning heat; I mean the apparent radiation of cold.

The first attempt to determine the effect of cold radiated from a mirror, was made by the Florentine academicians, in 1667, and is thus related in the essays of that academy, translated by Mr Waller. "We were willing to try if a concave glass set before a mass of 500lb. of ice, made any sensible repercussion upon a very nice thermometer of 400° placed in its focus. The truth is, it immediately began to subside; but by reason of its nearness to the ice, it was doubtful whether the direct or reflected rays of cold were more efficacious: upon this account we thought of covering the glass, (and whatever may be the cause) the spirit of wine did indeed presently begin to rise. For all this we dare not be positive, but there might be some other cause thereof, besides the want of reflection from the glass; since we were deficient in making all the trials necessary to clear the experiment."*

Gaertner, modeller to the king of Poland, repeated this experiment in a more satisfactory way in the year 1781. He contrived a set of wooden parabolic mirrors, which he gilded internally, with which he made various experiments. Among others was the following:—"If, instead of a hot body, cold water be put into the focus of the mirror, it gives out, even in summer, an agreeable coolness. But if, instead of cold water, I made use of ice, there was a considerable production of cold at the distance of ten or twenty paces."

This experiment was repeated by M. Pictet in somewhat a a different manner, and the account of it published by him in 1792, in his *Essai sur le Feu*. He took two concave mirrors indifferently polished, and having a focus at nine inches distance, and placed them opposite each other and 10½ feet asunder. A delicate air thermometer was put into one of the foci, and a matrass full of snow into the other; the thermometer sunk several degrees, and rose again when the matrass was removed. When nitric acid was poured on the snow the thermometer sunk several degrees lower.

* Waller's translation, p. 103.

This experiment has been considered by some as inexplicable, unless we admit the radiation of cold. The snow, it has been said, is not absolutely destitute of heat. It must therefore radiate some heat. Consequently its presence ought rather to raise than sink the thermometer. But this objection can be insurmountable to those only who have not taken the trouble to make themselves acquainted with the principles of Prevost's theory.

To explain what happens, we have only to recollect that heat is constantly radiating from all bodies and entering into all bodies, and that while the temperature continues unaltered, the rays of heat lost by radiation must be just compensated by the rays received. From the nature of radiation it is obvious that the intensity of the ray must be inversely as the square of the distance from the radiating object. A thermometer in a room may be considered as in the centre of a sphere of bodies constantly radiating heat upon it. The rays which it throws out just compensate those which it receives, and consequently its temperature remains unchanged. Suppose a mirror placed in its neighbourhood, it will intercept a considerable number of rays sent to the thermometer by the surrounding sphere. Indeed, if it be near enough and large enough, it will intercept a whole hemisphere of these rays. But this can produce no change upon the temperature of the thermometer, because the difference between the size of the mirror and of the hemisphere intercepted, will be exactly compensated by the smaller distance of the mirror. If we put the thermometer into the focus of the mirror, the effect will continue the same. The mirror will indeed collect the rays from one of the hemispheres, and reflect them upon the thermometer; but it will intercept the rays from the other hemisphere. The temperature of the thermometer will not be affected, because the number of rays sent to the thermometer being the same, it can make no difference whether these rays be supplied by reflection or radiation.

Suppose a second mirror to face the first. The cylinder of rays from the second hemisphere is now intercepted. But then all the rays which pass through its focus to its face are

reflected in the direction of the intercepted cylinder. Thus a cylinder of reflected rays is formed equal to and instead of the intercepted one. The temperature of the thermometer must still continue unaltered, because it continues to receive as many rays as it emits.

Let us now place a hot body in the focus of the second reflector. The cylinder from this reflector will now consist of an extraordinary number of rays, which will be collected by the other mirror and sent to the thermometer. The thermometer will now receive more rays than it emits, and consequently will rise. Substitute a matrass full of ice instead of the hot body in the focus of the second reflector, the consequence will be that the rays which would have otherwise passed through the focus of this reflector will be absorbed by the ice. Fewer rays will make their way to the other mirror and be collected into its focus. The thermometer will now radiate more heat than it receives, and of course will fall. When nitric acid is poured upon snow, the temperature sinks about 40°; it will therefore radiate still less heat than the ice. The thermometer therefore must sink still lower.*

There is an experiment by Count Rumford, which he considered as incompatible with the common opinions respecting radiation; but which admits of as easy an explanation as the cooling occasioned by the snow in Pictet's experiment. He took a conical metallic tube about 18 inches long, whose respective apertures were 1 inch, and 5 inches in diameter. The inside of the tube was polished so as to reflect heat powerfully and to radiate little. This tube being supported in a horizontal position, the focal ball of a differential thermometer was placed at the larger opening, while a matrass full of ice was put before the smaller opening. The thermometer did not sink, or sunk only a very little; but when the position of the ice and thermometer was reversed, by placing the former before the wide opening, and the latter in the narrow opening of the tube, the thermometer now sunk a considerable

* On this subject the reader may consult Mr Davenport's paper, Annals of Philosophy (1st series), p. 338, and Mr Crampton Holland's paper on the same subject in the Edin. Transactions, 179.

Chap. I. space, indicating a much more rapid decrease of temperature. This experiment is easily explained by the application of Prevost's theory. It is evident from the well-known laws of optics, that the incident rays of heat which are parallel to the axis of the tube are reflected in the form of cones, and cross in the axis. However, the largest hollow cylinder of incident rays will form the focus of greatest intensity; and a multitude of other rays will be brought by complex reflections to the same spot. The tube is, in fact, a truncated cone; for the smaller end has a diameter. The principal focus is not at the apex of the cone; but short of it, and we may suppose the thermometer placed in this point. Now, it is easy to see that the rays from a considerable portion of the surrounding sphere are intercepted by the tube; but this is compensated by the rays reflected by the inside of the tube, and falling upon the thermometer placed in the focus. Hence a thermometer placed at the narrow end of the tube undergoes no alteration whatever; but when the matrass with ice is placed at the wide end of the tube, it intercepts the incident rays which would have been reflected from the inside of the tube upon the thermometer. The thermometer now radiates more heat than it receives, and must therefore fall.

The radiation of heat being understood, we are enabled to explain, by means of it, a variety of natural phenomena of a very interesting nature. I may give an example or two to enable the reader to perceive the importance of this part of the doctrine of heat.

Cause of dew.

1. It is to the radiation of heat that we are indebted for the condensation of dew, to which plants are so much indebted for their nourishment, as was first satisfactorily explained by Dr Wells in his *Essay on Dew*, which constitutes one of the most beautiful examples of inductive reasoning in the English language. To understand the way in which dew is formed, it is necessary to know that water is capable of being converted into vapour at all temperatures from 32° to 212°: hence the atmosphere is seldom or never destitute of aqueous vapour. But the absolute quantity that can exist depends upon the temperature. At 32° it can contain only $\frac{1}{150}$th of its volume

of vapour, while at 52° it can contain $\frac{1}{88}$th of its volume. When air containing vapour diminishes in its temperature, a portion of its vapour is usually condensed into water. The amount of the diminution of temperature necessary to cause air to deposit moisture depends upon the quantity of vapour which it contains. If the quantity be as great as can exist at the given temperature, then the smallest diminution of temperature will occasion the deposition of humidity; but if air at 72° contain only as much vapour as it can retain at the temperature of 52°, it is obvious that it must be cooled below 52° before it begin to deposit moisture.

During the day a good deal of water is converted into vapour from the surface of lakes, seas, and rivers, and from the earth itself, and mixes with the atmosphere. The temperature of the atmosphere usually sinks considerably after sunset, and is often 20° or 30° colder than at the hottest part of the day: hence it must approach much nearer the point of depositing moisture than during the day. The greatest difference between the temperature of day and night takes place in this country in spring and autumn, and these are the seasons in which the most abundant dews are usually deposited. Dewy nights are usually clear; on cloudy nights dew seldom falls.

Many years ago, a curious set of experiments on dew was made by M. Dufay. He placed a glass cup in the middle of a silver basin, and left both in the open air during a dewy night. Next morning the silver basin was found dry; but the glass cup was wet with dew. When the experiment was reversed by placing a silver cup in the middle of a glass basin, the glass was still moist and the silver dry.* These, and many other similar experiments, remained unexplained till Dr Wells turned his attention to the subject. It is only necessary to say that the metals are bad radiators of heat, while glass is a good radiator: hence in a cloudless night the temperature of the glass exposed to the aspect of the sky will sink much lower than that of metals; it will cool the air in its neighbour-

* Mem. Par. 1736, p. 352.

Chap. I. hood more, and of course dew will be deposited on it in preference. Dr Wells found, as Mr Six had done before him, that a thermometer laid on a grass plot in a clear night sunk 6°, 8°, 13°, or even 20° lower than a thermometer hung at some height from the ground; because grass radiates heat well. In short, dew is deposited on those substances which radiate heat well; while it avoids, for an obvious reason, all bad radiators. These depositions do not take place on cloudy nights, because clouds radiate the heat back again, and thus prevent the temperature of good radiators from sinking much below that of the atmosphere.

Deposition of moisture on windows.

2. In frosty weather moisture is almost always condensed upon the inside of the windows of our apartments (during the night when the room is without fire), in the form of dew or hoar frost. The glass, being a good radiator, is speedily cooled below the temperature of the room. Vapour from the air in the apartment is consequently condensed upon it, and it assumes the form of dew or hoar frost according to the temperature of the glass. This condensation is much more abundant when the window shutters are closed than when they are left open. Because in the latter case the radiation from the different parts of the room upon the window supplies a considerable portion of the heat radiated by the glass, and prevents the temperature from sinking so low.

Formation of ice.

3. Dr Wells has shown, by a very happy induction, that the formation of artificial ice in India depends upon the radiation of heat from the surface of the water. The nights which answer best for making this ice are clear and calm. The water is put in shallow pans, and it is so placed that the heat from the ground cannot easily penetrate to it. The congelation is usually greatest before sun-rise, and as dew is copiously deposited it is obvious that no evaporation can take place from its surface; so that the explanation of this process formerly given by Dr Black, namely, the cold produced by evaporation, is fallacious.*

* From a passage in Josephus, De Bello Judaico, lib. iii. cap x., paragraph 7, it appears that the making of artificial ice was practised by the Jews. Describing the lake of Gennesareth, he says, Καὶ τὸ μὲν ὕδωρ [illegible]

4. In this country it is not uncommon for cold nights to occur in the month of May, when the fruit trees are in blossom. In such cases careful gardeners are in the habit of covering up those trees and plants that would be injured by the cold, with mats, and the precaution is generally successful. These mats act simply by preventing the heat from escaping from the plant by radiation; as they radiate as much or almost as much back, as they receive from the plant. The cold weather is usually over before the mat is cooled down so low by the action of the air as to cease to compensate to the plant for the loss of its heat by radiation.

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Use of mats to preserve plants.

CHAPTER II.

OF FLUIDITY.

All the substances in nature exist either in the state of solids, liquids, or gases, and the state depends upon the temperature to which they are exposed. There are many bodies, which in the common temperature of our atmosphere are solid; but which become *liquid* when exposed to a temperature sufficiently high. Tin and lead are solid metals; but they become liquid when exposed to a temperature below ignition. Sulphur melts at a still lower temperature; and water becomes liquid at so low a temperature that in this country it is very seldom in a solid state.

All solids may be fused, except the diamond.

All the solid bodies with which we are acquainted are capable of being converted into liquids by temperatures which we have it in our power to produce; with one remarkable exception, namely, the *diamond;* which it has been hitherto impossible to melt by the application of heat. But this does not constitute a true exception to the general law, that all solids

χιονος ἀφαιρεῖσθαι, ὅπερ θέρους νυκτος ποιεῖν ἔθος τοῖς ἐπιχωρίοις.' "The water *of the lake* exposed to the aspect of the sky during summer nights does not lose its snowy coldness; but resembles the ice which the inhabitants of the country are in the habit of making during summer."

Chap. II. may be rendered liquid by heat. Because the diamond, in consequence of some of its properties, cannot be exposed to a temperature so high as some other bodies can. Lime and magnesia were long considered as infusible by heat; and it was not till they were exposed to the very high temperature produced by burning a mixed stream of oxygen and hydrogen gases that their refractory nature was overcome. But the diamond cannot be exposed to such a test, because it is a combustible body, and catches fire, and is consumed at comparatively low temperatures. Nor can we expose it to a sufficiently high temperature to fuse it in close vessels; because with many substances, as iron, it combines when strongly heated with them, and because it is probably as refractory or more so than lime or magnesia, in vessels of which it cannot therefore be heated with any chance of fusion. Charcoal is in the same predicament with the diamond, because its nature is very nearly the same with that of the precious stone.*

There is a vast difference in the temperatures requisite to fuse different solid bodies. Lime, magnesia, alumina, and the other earthy bodies, together with silica, are so refractory that they have been fused only in minute particles by an ignited current of oxygen and hydrogen gases. Platinum cannot be fused in our furnaces, but it yields before the blowpipe urged by a stream of oxygen gas. Dr Clarke, by his oxygen and hydrogen blowpipe, melted 100 grains of platinum, and kept it in fusion for some minutes. Iron may be melted in a good draught furnace. Mr Mushet in this way melted pieces of iron about half-an-ounce in weight; and Sir George Mackenzie fixed its fusing point at 158° Wedgewood. Gold and copper melt easily in a wind furnace; and silver may be fused in a common fire urged by a pair of bellows. Zinc and antimony melt at about a red heat; lead, bismuth, and tin, at temperatures considerably lower; and the fusing point of mercury is so low that in this country we never see it in a

* Dr Silliman is of opinion that he melted charcoal by exposing it to the intense heat of Dr Hare's deflagrator. I repeated the experiment and obtained a microscopic bead; but it consisted of the earthy matters of which charcoal is never destitute.

solid state, except when we expose it to the action of a freezing mixture.

Liquids may be congealed.

It is no less true that all liquid bodies become solid when we expose them to a temperature sufficiently low. Thus *water* in frosty weather is changed into *ice*. Olive oil becomes solid, like tallow, when cooled down sufficiently low. Oil of turpentine requires a much more intense cold to freeze it; but the weather is occasionally cold enough even in this country to cause it to congeal. Neither mercury nor sulphuric ether ever freeze in Great Britain. But they would have congealed in Melville island, where Captain Parry wintered; for the thermometer at some little distance from the ship stood at —55°, which is about 10° degrees lower than the freezing point of sulphuric ether.

Except alcohol.

There is, however, one liquid which has never yet been frozen; at least the evidence that it has been is somewhat unsatisfactory. This liquid is *alcohol*. Common spirits freeze in severe colds; but absolute alcohol, or alcohol of the specific gravity 0·798, did not congeal when it was exposed by Mr Walker to a cold of —90°. It was indeed announced in 1813, in the Edinburgh newspapers, and repeated in the scientific journals, that Mr Hutton had succeeded, by a peculiar process, which he kept secret, in congealing it by reducing it to the temperature of —110°. But Mr Hutton's alcohol was obviously weak. Professor Leslie, if I understand him rightly, exposed absolute alcohol to a cold of —120° without observing any congelation.*

Alcohol excepted (if it really constitute an exception), all liquids are converted into solids when exposed to a sufficiently low temperature; and the diamond excepted, all solids are

* M. Bussy moistened a quantity of cotton with liquid sulphurous acid (this acid liquefies at about —10°), wrapped it round the bulb of a thermometer in the receiver of an air-pump, and exhausted the receiver. The thermometer sank to —90°·5. Alcohol of 0·85, exposed to the same action, became solid. It has been stated in a preceding part of this volume, that Muncke has calculated the point of congelation of absolute alcohol to be —140°; but Mr Kemp cooled it down to —163°, by means of solid carbonic acid, yet it still retained its liquid form.

Chap. II. converted into liquids when exposed to a sufficiently high temperature. Fluidity then is a very general effect of heat, and heat is obviously the efficient cause of fluidity. The addition of heat converts a solid into a fluid, while the abstraction of heat converts a fluid into a solid.

When we compare *fluidity* and *expansion*, we find this remarkable difference between them. Expansion takes place at every temperature, and is the constant concomitant of the addition of heat; but fluidity never takes place till bodies are heated up to a particular degree. This point is constant in every particular body, so as to characterize and distinguish it from every other. If the body be usually solid, we call the point at which it changes to a liquid the *melting point*; but, when the body in the usual temperature of the atmosphere is liquid, the point at which it changes its state and becomes solid, is called its *freezing point*. Thus we talk of the *melting* point of lead, sulphur, and tallow, and of the *freezing* point of water, oil, mercury.

As this point constitutes a remarkable fact in the history of bodies, it will be worth while to exhibit a tabular view of the melting points of those bodies which have been determined with tolerable accuracy.

Melting points.

	Fahrenheit.	Observer.
Cast iron melts at	2786°	Daniell
Silver with $\frac{1}{4}$ gold	2050	Princep*
Copper	1996	Daniell
Silver with $\frac{1}{10}$ gold	1920	Princep*
Silver	1873	Daniell
Silver	1830	Princep*
Brass	1869	Daniell
Antimony	810	Mortimer
Zinc	773	Daniell
Lead	612	Daniell
Lead	606·5	Crichton, jun.
Bismuth	497	Crichton, jun.
Tin	442	Crichton, sen.

* Phil. Trans. 1828, p. 79. These melting points were measured by the dilatation of air.

	Fahrenheit.	Observer.
1 atom lead + 1 atom tin	368°·6	Rudberg*
1 bismuth + 1 tin	289·4	Rudberg
1 zinc + 1 tin	399·2	Rudberg
Sulphur	218	Irvine
Bees' wax (bleached)	142	Nicholson
Spermaceti	112	Nicholson
Phosphorus	108	Thomson
Tallow	92	Nicholson
Olive oil	36	
Ice	32	
Milk	30	
Vinegar	28	
Sea water	27·5	Nairne
Blood	25	
Wines	20	
Oil of turpentine	14	Margueron
Sulphuric acid	1	Cavendish
Mercury	—39	Cavendish
Nitric acid	—45·5	Fourcroy and
Sulphuric ether	—46	Vauquelin.†

In certain circumstances liquids may be cooled down several degrees below their freezing point before they begin to congeal. The late Mr Crichton, of Glasgow, observed that tin melted in a crucible may be cooled down to 438° or 4° below its freezing point; but the instant it begins to congeal a thermometer plunged in it rises up to 442°, and continues

* Ann. de Chim. et de Phys. xlviii. 369.

† The following melting points of alloys of tin and lead (though they do not agree with those of Rudberg), determined by M. Kupffer, deserve to be recorded:—

Tin.	Alloy of	Lead.	Point of fusion.
1 atom	+	1 atom	466°
2	+	1	385
3	+	1	367
4	+	1	372
5	+	1	381

The compound of easiest fusion is that of 3 atoms tin and 1 atom lead. This compound changes its volume very little when the two metals are fused together. The melting point of lead, according to Kupffer, is 633°, that of tin 446°. See Ann. de Chim. et de Phys. xl. 302.

Chap. II. at that point till the whole tin has become solid. I have cooled water in thermometer tubes to 8°, and once to 5°, before it began to freeze. In all such cases, whenever the liquid begins to freeze, a thermometer plunged in it immediately starts up to the freezing point of the liquid, and continues at that point till the whole liquid has congealed. The instant that any solid body is dropt into water cooled down below the freezing point, it begins to congeal. The nature of the solid seems to be indifferent: a pin, a grain of salt, a piece of vegetable, animal or mineral substance. Agitation, or even wind, provided all solid matter be excluded, does not seem to occasion the commencement of congelation. These phenomena show an analogy between the separation of heat and elasticity. For the congelation is occasioned by the separation of a certain quantity of heat from the liquid.

Effect of salts in altering the freezing of water.

When salts are dissolved in water, it is well known that its freezing point is in most cases lowered. Thus sea-water does not freeze so readily as pure water. The experiments of Sir Charles Blagden have given us the point at which a considerable number of these solutions congeal. The result of his trials may be seen in the following table: the first column contains the names of the salts; the second the quantity of salt, by weight, dissolved in 100 parts of water; and the third, the freezing point of the solution :—*

Names of Salts.	Proportion.	Freezing point.
Common salt . .	25 . .	4
Sal ammoniac . .	20 . .	8
Rochelle salt . . .	50 . .	21
Epsom salt, or sulphate of magnesia . .	41·6 . .	25·5
Nitre	12·5 . .	26
Sulphate of iron . .	41·6 . .	28
Sulphate of zinc . .	53·3 . .	28·5

From this table it appears that common salt is by far the most efficacious in lowering the freezing point of water. A solution of 25 parts of salt in 100 of water freezes at 4°. These solutions, like pure water, may be cooled down considerably

* See Phil. Trans. 1788, p. 277.

below their freezing point without congealing; and in that case the congelation is produced by means of ice, just as in common water, though more slowly.

When the proportion of the same salt held in solution by water is varied, it follows, from Sir Charles Blagden's experiments, that the freezing point is always proportional to the quantity of the salt. For instance, if the addition of $\frac{1}{10}$th of salt to water lowers its freezing point 10°, the addition of $\frac{2}{10}$ths will lower it 20°: hence, knowing from the preceding table the effect produced by a given proportion of salt, it is easy to calculate what the effect of any other proportion will be. The following table exhibits the freezing points of solutions of different quantities of common salt in 100 parts of water, as ascertained by Blagden's trials, and the same points calculated on the supposition that the effect is as the proportion of salt:—

Quantity of salt to 100 of water.	Freezing point by experiments.	Do. by calculation.
3·12	28 +	28·5
4·16	27·5	27·3
6·25	25·5	25
10·00	21·5	20·75
12·80	18·5	17·6
16·1	13·5	14
20	9·5	9·8
22·2	7·2	7
25	4	4

The experiments of Despretz* do not show the same regularity, as will appear from the following tables, showing the freezing points of solutions of anhydrous carbonates of potash and soda, and chlorides of sodium and calcium, in different proportions in water:—

1. CARBONATE OF POTASH.

Salt dissolved in 1000 water.	Freezing point.
6·173	31°·66
12·346	31·325
24·692	30·605

* Poggendorff's Annalen, xli. 495.

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Salt dissolved in 1000 water.	Freezing point.
37·039 . . .	31°·703
74·078 . . .	31·532
148·156 . . .	30·488

2. CARBONATE OF SODA.

Salt dissolved in 1000 water.	Freezing point.
6·173 . . .	31°·248
12·346 . . .	30·816
24·692 . . .	29·970

3. CHLORIDE OF SODIUM.

Salt dissolved in 1000 water.	Freezing point.
6·173 . . .	31°·712
12·346 . . .	30·402
24·692 . . .	29·462
37·039 . . .	28·184
74·078 . . .	24·188
148·156 . . .	14·440

4. CHLORIDE OF CALCIUM.

Salt dissolved in 1000 water.	Freezing point.
6·173 . . .	31°·284
12·346 . . .	30·726
24·692 . . .	30·146
37·039 . . .	29·102
74·078 . . .	25·592
148·156 . . .	15·962

Of strong acids.

3. The strong acids, namely, sulphuric and nitric, whi are in reality compounds, containing various proportions water according to their strength, have been shown by M Cavendish, from the experiments of Mr Macnab, to vary a remarkable manner in their point of congelation, accordi to circumstances. The following are the most importa points respecting the freezing of these bodies that have be ascertained :—

When these acids, diluted with water, are exposed to col the weakest part freezes, while a stronger portion remai liquid; so that by the action of cold, they are separated in two portions, differing very much in strength. This has be

termed by Mr Cavendish the *aqueous* congelation of these bodies. Chap. II.

When they are very much diluted, the whole mixture, when exposed to cold, undergoes the *aqueous* congelation; and in that case, it appears from Blagden's experiments, that the freezing point of water is lowered by mixing it with acid rather in a greater ratio than the increase of the acid. The following table exhibits the freezing point of mixtures of various weights of sulphuric acid, of the density 1·837 (temperature 62°), and of nitric acid of the density 1·454, with 100 parts of water :—

Sulphuric Acid.		Nitric Acid.	
Proportion of acid.	Freezing point.	Proportion of acid.	Freezing point.
10	24·5	10	22
20	12·5	20	10·5
25	7·5	23·4	7*

The concentrated acids themselves undergo congelation when exposed to a sufficient degree of cold; but each of them has a particular strength at which it congeals most readily. When either stronger or weaker, the cold must be increased. The following table, calculated by Mr Cavendish from Mr Macnab's experiments, exhibits the freezing points of nitric acid of various degrees of strength :—†

Nitric acid.

Specific gravity.	Strength.	Freezing point.	Difference.
1·42	568	—45·5	+15·4
1·41	538	—30·1	+12
1·39	508	—18·1	+ 8·7
1·37	478	— 9·4	+ 5·3
1·35	448	— 4·1	+ 1·7
1·33	418	— 2·4	— 1·8
1·31	388	— 4·2	— 5·5
1·28	358	— 9·7	— 8
1·26	328	—17·7	—10
1·23	298	—27·7	

The following table exhibits the freezing points of sulphuric acid of various strengths :—‡

* Phil. Trans. 1788, p. 303.

† The strength is indicated by the quantity of marble necessary to saturate 1000 parts of the acid. Phil. Trans. 1788, p. 174.

‡ Phil. Trans. 1788, p. 181.

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Sulphuric acid.

Specific gravity.	Strength.	Freezing point.
1·837	977	+1
1·827	918	—26
1·770	846	+42
1·670	758	—45

Mr Keir had previously ascertained that sulphuric acid of the specific gravity 1·780 (at 60°) freezes most easily, requiring only the temperature of 46°. This agrees nearly with the preceding experiments, as Mr Cavendish informs us that sulphuric acid of that specific gravity is of the strength 848. From the preceding table we see, that besides this strength of easiest freezing, sulphuric acid has another point of contrary flexure at a superior strength; beyond this, if the strength be increased, the cold necessary to produce congelation begins again to diminish.

Dr Black's opinion.

4. Before Dr Black began to deliver his chemical lectures in Glasgow, in 1757, it was universally supposed that solids were converted into liquids by a small addition of heat after they have been once raised to the melting point, and that they returned again to the solid state on a very small diminution of the quantity of heat necessary to keep them at that temperature. An attentive view of the phenomena of liquefaction and solidification gradually led this sagacious philosopher to observe their inconsistence with the then received opinions, and to form another, which he verified by direct experiments. He drew up an account of his theory, and the proofs of it, which was read to a literary society in Glasgow on April 23, 1762;* and every year after, he gave a detailed account of the whole doctrine in his lectures.

That fluidity is occasioned by latent heat.

The opinion which he formed was, that when a solid body is converted into a liquid, a much greater quantity of heat enters into it than is perceptible immediately after by the thermometer. This great quantity of heat does not make the body apparently warmer, but it must be thrown into it in order to convert it into a liquid; and this great addition of heat is the principal and most immediate cause of the fluidity induced.

* Black's Lectures, preface, p. 38.

On the other hand, when a liquid body assumes the form of a solid, a very great quantity of heat leaves it without sensibly diminishing its temperature; and the state of solidity cannot be induced without the abstraction of this great quantity of heat. Or, in other words, whenever a solid is converted into a fluid, it combines with a certain dose of heat, without any augmentation of its temperature; and it is this dose of heat which occasions the change of the solid into a fluid. When the fluid is converted again into a solid, the dose of heat leaves it without any diminution of its temperature; and it is this abstraction which occasions the change. Thus the combination of a certain dose of heat with ice causes it to become water, and the abstraction of a certain dose of caloric from water causes it to become ice. Water, then, is a compound of ice and heat; and in general, all fluids are combinations of the solid to which they may be converted by cold, and a certain dose of heat.

Such is the opinion, concerning the cause of fluidity, taught by Dr Black as early as 1762. Its truth was established by the following experiments:—

Proved by experiment.

First. If a lump of ice, at the temperature of 22°, be brought into a warm room, in a very short time it is heated to 32°, the freezing point. It then begins to melt; but the process goes on very slowly, and several hours elapse before the whole ice is melted. During the whole of that time its temperature continues at 32°; yet as it is constantly surrounded by warm air, we have reason to believe that heat is constantly entering into it. Now, as none of this heat is indicated by the thermometer, what becomes of it, unless it has combined with that portion of the ice which is converted into water, and unless it is the cause of the melting of the ice?

Dr Black took two thin globular glasses, four inches in diameter, and very nearly of the same weight. Both were filled with water; the contents of the one were frozen into a solid mass of ice, the contents of the other were cooled down to 33°; the two glasses were then suspended in a large room at a distance from all other bodies, the temperature of the air being 47°. In half-an-hour the thermometer placed in the water glass rose from 33° to 40°, or seven degrees: the ice

Chap. II. was at first four or five degrees colder than melting snow; but in a few minutes the thermometer applied to it stood at 32°. The instant of time when it reached that temperature was noted, and the whole left undisturbed for ten hours and a half. At the end of that time the whole ice was melted, except a very small spongy mass, which floated at the top and disappeared in a few minutes. The temperature of the ice-water was 40°.

Thus 10½ hours were necessary to melt the ice and raise the product to the temperature of 40°. During all this time, it must have been receiving heat with the same celerity as the water glass received it during the first half-hour. The whole quantity received then was 21 times 7, = 149°; but its temperature was only 40°: therefore 139 or 140 degrees had been absorbed by the melting ice, and remained concealed in the water into which it had been converted, its presence not being indicated by the thermometer.*

That heat is actually entering into the ice, is easily ascertained by placing the hand or a thermometer under the vessel containing it. A current of cold air may be perceived descending from it during the whole time of the process.

But it will be said, perhaps, that the heat which enters into the ice does not remain there, but is altogether destroyed. This opinion is refuted by the following experiment:—

Second. If when the thermometer is at 22°, we expose a vessel full of water at 52° to the open air, and beside it another vessel full of brine at the same temperature, with thermometers in each; we shall find that both of them gradually lose heat, and are cooled down to 32°. After this the brine (which does not freeze till cooled down to 4°) continues to cool without interruption, and gradually reaches 22°, the temperature of the air; but the pure water remains stationary at 32°. It freezes, indeed, but very slowly; and during the whole process its temperature is 32°. Now, why should the one liquid refuse all of a sudden to give out heat, and not the other? Is it not much more probable that the water, as it freezes, gradually gives out the heat which it had absorbed during its

* Black's Lectures, i. 120.

liquefaction; and that this evolution maintains the temperature of the water at 32°, notwithstanding what it parts with to the air during the whole process? We may easily satisfy ourselves that the water while congealing is constantly imparting heat to the surrounding air; for a delicate thermometer suspended above it is constantly affected by an ascending stream of air less cold than the air around.* The following experiment, first made by Fahrenheit, and afterwards often repeated by Dr Black and others, affords a palpable evidence, that such an evolution of heat actually takes place during congelation:—

Third. If, when the air is at 22°, we expose to it a quantity of water in a tall beer glass, with a thermometer in it and covered, the water gradually cools down to 22° without freezing. It is therefore 10° below the freezing point. Things being in this situation, if the water be shaken, part of it instantly freezes into a spongy mass, and the temperature of the whole instantly rises to the freezing point; so that the water has acquired ten degrees of heat in an instant. Now, whence came these ten degrees? Is it not evident that they must have come from that part of the water which was frozen, and consequently that water in the act of freezing gives out heat?

From a good many experiments which I have made on water in these circumstances, I have found reason to conclude, that the quantity of ice which forms suddenly on the agitation of water, cooled down below the freezing point, bears always a constant ratio to the coldness of the liquid before agitation. Thus I find that when water is cooled down to 22°, very nearly $\frac{1}{14}$th of the whole freezes;† when the previous temperature is 27°, about $\frac{1}{28}$th of the whole freezes. I have not been able to make satisfactory experiments in temperatures lower than 22°; but from analogy I conclude, that for every five degrees of diminution of temperature below the freezing point, without congelation, $\frac{1}{28}$th of the liquid freezes suddenly on agitation. Therefore, if water could be cooled down 28 times five degrees

* Black's Lectures, i. 127.

† A medium of several experiments.

Chap. II. below 32° without congelation, the whole would congeal instantaneously on agitation, and the temperature of the ice would be 32°. Now it deserves attention that $5 \times 28 = 140$, gives us precisely the quantity of heat which, according to Dr Black's experiments, enters into ice in order to convert it into water: hence it follows, that in all cases when water is cooled down below 32°, it loses a portion of the heat which is necessary to constitute its liquidity. The instant that such water is agitated, one portion of the liquid seizes upon the quantity of caloric in which it is deficient, at the expense of another portion, which, of course, becomes ice. Thus, when water is cooled down to 22°, every particle of it wants 10° of the heat necessary to keep it in a state of liquidity. Thirteen parts of it seize ten degrees each from the fourteenth part. These thirteen of course acquire the temperature of 32°; and the other part being deprived of $10 \times 13 = 130$, which, with the ten degrees that it had lost before, constitute 140°, or the whole of the heat necessary to keep it fluid, assumes of consequence the form of ice.

Fourth. If these experiments should not be considered as sufficient to warrant Dr Black's conclusion, the following, for which we are indebted to the same philosopher, puts the truth of his opinion beyond the reach of dispute. He mixed together given weights of ice at 32° and water at 190° of temperature. The ice was melted in a few seconds, and the temperature produced was 53°. The weight of the ice was 119 half-drams;

That of the hot water . .	135
of the mixture . . .	254
of the glass vessel . .	16

Sixteen parts of glass have the same effect in heating cold bodies as eight parts of equally hot water. Therefore, instead of the 16 half-drams of glass, eight of water may be substituted, which makes the hot water amount to 143 half-drams.

In this experiment there were 158 degrees of heat contained

in the hot water to be divided between the ice and water. Chap. II.
Had they been divided equally, and had the whole been afterwards sensible to the thermometer, the water would have retained $\frac{143}{262}$ parts of this heat, and the ice would have received $\frac{119}{262}$ parts. That is to say, the water would have retained 86°, and the ice would have received 72°: and the temperature after mixture would have been 104°. But the temperature by experiment is found to be only 53°; the hot water lost 137°, and the ice only received an addition of temperature equal to 21°. But the loss of 18° of temperature in the water is equivalent to the gain of 21° in the ice. Therefore 158° — 18° = 140° of heat have disappeared altogether from the hot water. These 140° must have entered into the ice, and converted it into water without raising its temperature.*

In the same manner, if we take any quantity of ice, or, (which is the same thing) snow at 32°, and mix with it an equal weight of water at 172°, the snow instantly melts, and the temperature of the mixture is only 32°. Here the water is cooled 140°, while the temperature of the snow is not increased at all; so that 140° of heat have disappeared. They must have combined with the snow; but they have only melted it without increasing its temperature: hence it follows irresistibly, that ice, when it is converted into water, absorbs and combines with heat.

It is rather difficult to ascertain the precise number of degrees of heat that disappear during the melting of ice: hence different statements have been given. Mr Cavendish, who informs us that he discovered the fact before he was aware that it was taught by Dr Black, states them at 150°;† Wilke at 130°; Black at 140°; and Lavoisier and Laplace, at 135°. The mean of the whole is very nearly 140°.

Latent heat of water.

Water, then, after being cooled down to 32°, cannot freeze till it has parted with 140° of heat; and ice, after being heated to 32°, cannot melt till it has absorbed 140° of heat. This is the cause of the extreme slowness of these operations. With regard to water, then, there can be no doubt that it owes its

* Black's Lectures, i. 123. † Phil. Trans. 1733, p. 313.

Chap. II. fluidity to the caloric which it contains, and that the caloric necessary to give fluidity to ice is equal to 140°.

To the quantity of heat which thus occasions the fluidity of solid bodies by combining with them, Dr Black gave the name of *latent heat*, because its presence is not indicated by the thermometer: a term sufficiently expressive; but other philosophers have rather chosen to call it *caloric of fluidity*.

Dr Black and his friends ascertained also, by experiment, that the fluidity of melted wax, tallow, spermaceti, and metals, is owing to the same cause. Landriani proved that this is the case with sulphur, alum, nitre, and several of the metals;* and it has been found to be the case with every substance hitherto examined. We may consider it therefore as a general law, that whenever a solid is converted into a fluid, it combines with heat, and that this is the cause of its fluidity.

Latent heat of other bodies.

5. The only experiments made to determine the latent heat of other bodies besides water, are those of Dr Irvine,† his son,‡ and M. Rudberg.§ Rudberg indeed states, that Dr Black had ascertained the latent heat of tin to be 500°, and that of wax to be 175°. But these experiments were made by Irvine, not Black; and Rudberg has mistaken the meaning of Dr Irvine's conclusions. 500° do not constitute the number of degrees that the same weight of water would be raised; but the increase of temperature which the latent heat of tin would produce in the tin if it were to be thrown into the solid tin without melting it. The same remark applies to his experiments on wax.

Irvine's experiments were made by pouring the melted bodies (reduced as nearly as possible to the melting point) into water, and observing the rise of temperature in that liquid. Rudberg's method was to put the melted metals into a vessel surrounded with snow, to prevent any access of heat from without, and observing the time that each took to cool 10°, (including the point of becoming solid,) compared with the time to produce the same diminution of temperature in me

* Jour. de Phys. xxv. † Black's Lectures, i. 187.
‡ Nicholson's Jour. ix. 45. § Ann. de Chim. et de Phys. xlviii. 353

cury placed in the same situation. From this time he calculated the latent heat. Chap. II.

Let the weight of mercury $= m$; its specific heat $= c$. The heat lost in sinking ten degrees will be $10\ mc$.

The other metal loses not only its latent heat, but also (like the mercury) the heat which escapes while the thermometer is sinking 10°.

Let its weight be M, its latent heat $= L$, its specific heat at the moment of solidification $= C$. Its total loss of heat will be $= (M\,L + 10\ C)$.

Let t be the time which the mercury takes to sink 10°, and T the corresponding time; when the crucible contains the other metal, we have $M\,(L + 10\ C) : 10\ mc = T : t$; or, if we wish to reckon the loss of heat of the crucible itself, its mass being $= \mu$, and its specific heat c'',

$$M\,(L + 10\ C) + 10\ \mu c'' : 10\ (mc + \mu c'') = T : t.$$

Hence

$$L = \frac{10\,(mc + \mu c'')\,T}{M\,t} - 10\left(C + \frac{\mu}{M}\,c''\right).$$

It is obvious that this formula is not quite exact; because the crucible, when filled with mercury, is constantly cooling, while the thermometer is sinking the 10°; whereas it remains unaltered during the greatest part of that time when the other metal is in it. But the error from this source cannot be great.

The following table exhibits the results obtained by these experiments:—

LATENT HEATS.

	Referred to the bodies.	Referred to water.
Ice	140°	140°
Sulphur	143·68	27·14
Spermaceti	145	
Lead	162	5·6
Ditto		10·93*
Bees' wax	175	

* Rudberg.

Chap. II.

	Referred to the bodies.	Referred to water.
Zinc	493	48·3
Tin	500	33
Ditto		24·85 *
Bismuth	550	23·25

The latent heat of spermaceti, wax, and tin, were determined by Dr Irvine; that of the rest by his son. The latent heat in the second column expresses the degrees by which it would have increased the temperature of each of the bodies respectively when solid, except in the case of spermaceti and wax; in them it expresses the increase of temperature which would have been produced upon them while fluid.

6. Dr Black has rendered it exceedingly probable also, or rather he has proved by his experiments and observations, that the softness of such bodies as are rendered plastic by heat depends upon a quantity of latent heat which combines with them. Metals also owe their malleability and ductility to the same cause: hence the reason that they become hot and brittle when hammered.

It is greatly to be regretted that this interesting subject has not been farther prosecuted. Meanwhile the knowledge we have already gained will enable us to account for many important natural phenomena. It will be worth while to give a few examples.

Freezing mixtures explained.

1. It is well known that there are certain substances which, when mixed together, induce a considerable cold. These substances are used for the purpose under the name of *freezing mixtures*. *Snow* and *salt* are two such substances. If we mix them together in about equal quantities in a glass or stoneware vessel, a thermometer plunged into the mixture sinks down to zero, and continues at this low temperature for a considerable time. There is reason to believe that this important experiment, which has added so much to the enjoyment of the rich in hot climates, was first made by Sanctorio, about the close of the sixteenth century. Nitre and snow had been already used as a freezing mixture by the Italian pastry-

* Rudberg.

cooks; but Sanctorio informs us in his commentary on Avicenna that he produced the same effect by substituting common salt for nitre, in the proportion of three parts snow to one part of salt; and that he had repeatedly performed the experiment before numerous audiences.

A still greater diminution of temperature is produced by pouring nitric acid, previously diluted with water, upon snow. And when dry snow and dry chloride of calcium (retaining its water of crystallization) in the proportion of one part of the former to two parts of the latter are mixed, so great a cold is produced that mercury may easily be frozen by it even in a warm room.

The first person who made experiments on freezing mixtures was Fahrenheit. But the subject was much more completely investigated by Mr Walker, in various papers published in the Philosophical Transactions from 1787 to 1801. Several curious additions have been made by Professor Lowitz. The experiments of Lowitz have been repeated and extended by Mr Walker.* The result of these experiments may be seen in the following tables, which I transcribe from a paper with which I have been favoured by Mr Walker :—

* Phil. Trans. 1801, p. 120.

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TABLE I.—*Frigorific Mixtures without Ice.*

Mixtures.	Parts.	Thermometer sinks.	Degree of cold produced.
Muriate of ammonia Nitrate of potash Water	5 5 16	From + 50° to + 10°.	40
Muriate of ammonia Nitrate of potash Sulphate of soda Water	5 5 8 16	From + 50° to + 4°.	46
Nitrate of ammonia Water	1 1	From + 50° to + 4°.	46
Nitrate of ammonia Carbonate of soda Water	1 1 1	From + 50° to — 7°.	57
Sulphate of soda Diluted nitric acid	3 2	From + 50° to — 3°.	53
Sulphate of soda Muriate of ammonia Nitrate of potash Diluted nitric acid	6 4 2 4	From + 50° to — 10°.	60
Sulphate of soda Nitrate of ammonia Diluted nitric acid	6 5 4	From + 50° to — 14°.	64
Phosphate of soda Diluted nitric acid	9 4	From + 50° to — 12°.	62
Phosphate of soda Nitrate of ammonia Diluted nitric acid	9 6 4	From + 50° to — 21°.	71
Sulphate of soda Muriatic acid	8 5	From + 50° to 0°.	50
Sulphate of soda Diluted sulphuric acid	5 4	From + 50° to + 3°.	47

N. B.—If the materials are mixed at a *warmer* temperature than that expressed in the table, the effect will be proportionally *greater*; thus if the most powerful of these mixtures be made, when the air is + 85°, it will sink the thermometer to + 2°.

[ABLE II.—*Frigorific Mixtures with Ice.*

Mixtures.	Parts.	Thermometer sinks.		Degree of cold produced.
pounded ice of soda . .	2 1	*From any Temperature.*	to — 5°.	*
pounded ice of soda . . of ammonia .	5 2 1		to — 12°.	*
pounded ice of soda . . of ammonia . f potash . .	24 10 5 5		to — 18°.	*
pounded ice f soda . . f ammonia .	12 5 5		to — 25°.	
. . . . ulphuric acid	3 2	From + 32° to — 23°.		55
. . . . acid . . .	8 5	From + 32° to — 27°.		59
. . . . itric acid .	7 4	From + 32° to — 30°.		62
. . . . of lime . .	4 5	From + 32° to — 40°.		72
. . . . ariate of lime	2 3	From + 32° to — 50°.		82
. . . .	3 4	From + 32° to — 51°.		83

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TABLE III.—*Combinations of Frigorific Mixtures.*

Mixtures.	Parts.	Thermometer sinks.	Degrees of cold produced.
Phosphate of soda . Nitrate of ammonia . Diluted nitric acid .	5 3 4	From 0° to — 34°.	34
Phosphate of soda . Nitrate of ammonia . Diluted mixed acids .	3 2 4	From — 34° to — 50°.	16
Snow . . . . . . Diluted nitric acid .	3 2	From 0° to — 46°.	46
Snow . . . . . . Diluted sulph. acid Diluted nitric acid	8 3 } 3 }	From — 10° to — 56°.	46
Snow . . . . . . Diluted sulphuric acid	1 1	From — 20° to — 60°.	40
Snow . . . . . . Muriate of lime . .	3 4	From + 20° to — 48°.	68
Snow . . . . . . Muriate of lime . .	3 4	From + 10° to — 54°.	64
Snow . . . . . . Muriate of lime . .	2 3	From — 15° to — 68°.	53
Snow . . . . . . Cryst. muriate of lime	1 2	From 0° to — 66°.	66
Snow . . . . . . Cryst. muriate of lime	1 3	From — 40° to — 73°.	33
Snow . . . . . . Diluted sulphuric acid	8 10	From — 68° to — 91°.	23

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In order to produce these effects, the salts employed must fresh crystallized, and newly reduced to a very fine powder. he vessels in which the freezing mixture is made should be ry thin, and just large enough to hold it, and the materials uld be mixed together as quickly as possible. The materials be employed in order to produce great cold ought to be t reduced to the temperature marked in the table, by placing m in some of the other freezing mixtures; and then they to be mixed together in a similar freezing mixture. If, instance, we wish to produce a cold $=-46°$, the snow and ited nitric acid ought to be cooled down to $0°$, by putting vessel which contains each of them into the first freezing ture in the second table before they are mixed together. a still greater cold is required, the materials to produce it to be brought to the proper temperature by being previ-ly placed in the second freezing mixture. This process is be continued till the required degree of cold has been pro-ed.*

It is sometimes a matter of convenience to produce cold n we have no snow or ice. This may be done by dis-ing rapidly any salt containing much water of crystalliza-. Glauber salt is one of the most convenient, and dilute iatic acid or sulphuric acid the most suitable liquid to dis-e it in. The following experiments by Professor Bischof, onn, will enable the reader to judge of the degree of cold h may be produced in this way:—

	Mixture.			Sinks the thermometer. From	To	Cold produced.
)	500	gr.	Sulphuric acid			
	500	gr.	Water			
	1250	gr.	Glauber salt	54°·5	16°·25	38·°25
)	500	gr.	Sulphuric acid			
	750		Water			
	1560		Glauber salt	54·5	22·44	32·06
)	500	gr.	Sulphuric acid			
	635		Water			
	1400		Glauber salt	54·5	20·19	34·31
)	500	gr.	Sulphuric acid			
	208		Water			
	885		Glauber salt	54·5	14	40·5

Cold produced by dissolving sulphate of soda in dilute sulphuric acid.

* Walker, Phil. Trans. 1795.

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	Mixture.			Sinks the thermometer From	To	Cold produced.
(5)	500	gr.	Sulphuric acid			
	500		Water			
	1250		Glauber salt	54·5	10⅝	44—
(6)	500	gr.	Sulphuric acid			
	300		Water			
	990		Glauber salt	54·5	7¼	47·25
(7)	500	gr.	Sulphuric acid			
	250		Water			
	937		Glauber salt	54·5	7¼	47·25
(8)	500	gr.	Sulphuric acid			
	500		Water			
	1000		Glauber salt	54·5	7¼	47·25
(9)	500	gr.	Sulphuric acid			
	416		Water			
	1150		Glauber salt	54·5	6⅛	48·4
(10)	500	gr.	Sulphuric acid			
	333		Water			
	1040		Glauber salt	54·5	5	49·5

The acid and water must be previously mixed and allowed to cool, before the glauber salt be added. It should be in powder, but retaining all its water of crystallization.*

Theory of freezing mixtures.

All substances which produce cold on mixture, act chemically on each other. They are either both solid, or at least one of them is solid, and they begin to liquefy as soon as they are mixed. Indeed every mixture that generates cold, contains a considerable quantity of water in a solid state, and consequently destitute of its latent heat. This is the case with snow, which is almost always a constituent of every freezing mixture. Chloride of calcium in crystals contains more than half its weight of solid water. Now, whenever the two substances so mixed begin to act on each other, the tendency to liquefaction is greatly increased. The degree of cold is always proportional to the rapidity of the liquefaction: hence snow and chloride of calcium or potash, which liquefy immediately on mixture, produce a much greater cold than snow and common salt, which liquefy very slowly. But the cold produced by the former mixtures continues a much shorter time than that produced by the latter.

In proportion as the solid bodies become liquid they must

* Schweigger's Iahrbuch, xxii. 370.

absorb latent heat. This they can only obtain from the bodies in the neighbourhood: hence they absorb heat with avidity from all bodies in contact with them. But when a body absorbs heat from those in contact with it, we say that it is cold; and the more rapidly it absorbs heat the colder it is.

2. There are many salts which dissolve in hot water in considerably greater quantity than in cold water. If such hot solutions be set aside till they cool, the excess of salt which they contain will be deposited in crystals, and no more will remain in solution than cold water is capable of taking up. But in some rare instances it happens that if the hot solution be put into a phial and corked up and allowed to cool without agitation, no crystals will be deposited, so that we obtain a cold solution holding more salt than water of the same temperature is capable of dissolving. But the moment we draw out the cork from the phials containing them, a quantity of the salt suddenly crystallizes, and the temperature of the liquid at the same time rises. Carbonate of soda and sulphate of soda constitute two such salts.

Heat produced by sudden crystallization.

To form a solution of carbonate of soda which may be cooled down to 50° without crystallizing in a close phial, but which deposits crystals when the cork is drawn, we have only to dissolve one ounce of anhydrous carbonate in 4·22 ounces of hot water. When such a solution is agitated by drawing the cork at the temperature of 50°, a copious precipitate of small crystals in the form of stars takes place; and the temperature of the solution is elevated 14°. The crystals deposited in this case, supposing them anhydrous, weigh 123·15 grains, the water of crystallization belonging to which is 223·6 grains. The whole weight of the liquid and phial (substituting for the glass the requisite weight of water) was 2149·5 grains. Of which 223·6 grains make $\frac{1}{9\cdot 61}$ part. Now, if this water of crystallization had given out the whole of its latent heat, this evolution would have raised the temperature to 14°·96. Thus we see that the elevation of temperature in this case is owing to the latent heat given out by the water when it becomes solid by uniting to the precipitated salt.

To form a solution of sulphate of soda possessing the

Chap. II. requisite properties, we must dissolve 51 parts of the crystals in 49 parts of hot water. When such a solution is allowed to cool in a corked phial, if we draw the cork it begins immediately to crystallize on the surface, and the crystals shoot slowly till they reach the bottom of the phial. The whole liquid appears converted into fibrous crystals; but, in reality, two-thirds of the salt are deposited in crystals, and one-third remains in solution. When the weight of the solution of sulphate of soda was 2118 grains, and that of the phial 103 grains, the increase of temperature was 24°. The crystals of salt deposited weighed 713 grains, the water of crystallization of which amounts to 399 grains: the weight of the solution and phial are equivalent to 1752 grains of water. Now 399 is about $\frac{1}{4 \cdot 39}$ of 1752. And $24^{\circ} \times 4 \cdot 39 = 105^{\circ} \cdot 36$. This is $34^{\circ}\frac{3}{5}$ less than the latent heat of the water of crystallization. The reason of this is, that the crystals deposited at first do not amount to 713 grains, but only to 530 grains; the evolution of heat preventing the 183 residual grains from being deposited, till the liquid cools. Now, the water of crystallization of 530 grains of the salt is only 300 grains, constituting $\frac{1}{5 \cdot 83}$ of the whole. Now, $24^{\circ} \times 5 \cdot 83 = 139^{\circ} \cdot 92 =$ very nearly to the latent heat of the water of crystallization. Thus we see that the heat evolved in these cases of crystallization is owing chiefly, if not entirely, to the water of crystallization giving out its latent heat at the instant it becomes solid.*

The reason why these two salts remain in solution, must be the strong affinity which the water has for heat. In like manner, both sulphur and phosphorus often remain liquid at the common temperature of the atmosphere: but when touched with a rod, they give out heat and become solid.

3. Iron, copper, gold, silver, and one or two other metals, are both malleable and ductile. They may be flattened under the hammer and drawn out into wire, by being squeezed through holes drilled in a steel plate. Now, when any of

* The specific heat of a saturated solution of carbonate of soda is about 0·75; that of a saturated solution of sulphate of soda about 0·73. The specific heat of the phial was reckoned 0·2.

se metals have been hammered for some time so as to in- Chap. III.
ase its surface, to a certain amount it becomes brittle. If
still continue to hammer it, instead of extending farther,
cracks and splits and breaks in pieces. To restore the Annealing explained.
lleability, we must heat the piece of metal in the fire and
w it to cool slowly. This process is called *annealing.*

Now, there is a circumstance which always accompanies
hammering of metals, or the drawing them out into wires
plates. They become hot; sometimes even red hot, and
ally so hot that they burn the fingers. Thus it appears
t the forcing out of heat occasions brittleness in these
als. And to restore the malleability and ductility, we
t heat them artificially, and allow them to cool slowly;
t is to say, we must again restore the heat which had been
ed out. Thus malleability and ductility would seem to
pend upon a certain quantity of latent heat in the metals
ich possess these qualities; and the process of annealing is
ly the restoration of this latent heat after it has been
arated. If annealing consisted in merely heating metals
hout adding any foreign heat, there is no visible reason
y an iron rod which has been heated to redness by hammer-
g should require annealing; for it is already as hot as is
uired by the annealing process. Yet, it is well known,
t iron which has been made red hot by hammering, has
its malleability, and that it cannot be hammered again till
has been annealed.

CHAPTER III.

OF VAPORIZATION.

vaporization is meant the property which heat has of
nging liquid bodies into *vapours.* I shall divide this chap-
into three sections. In the first we shall treat of *vapours,*
he second of *gases,* and in the third of *spontaneous evapor-*
n.

Chap. III.

SECTION I.—OF VAPOURS.

Nature of vapour.

Vapour is an extremely rare, light, and expansible bo like *air;* and capable, like it, of being easily reduced into l space by external pressure, and resisting, like it, the force whi thus compresses it. Suppose a single tea-spoonful of wat to be put into a glass globe capable of holding several gallo and exhausted of air. If heat enough be applied to the glob the water gradually diminishes in bulk, and at last disappea so that the vessel will appear empty; yet it is complete filled with the water now existing in the state of transpare and invisible vapour. By increasing the heat, we increa the expansive force of this vapour; and we may increase till it burst the globe and shatter it in pieces.

1. A glass jar capable of holding 100 cubic inches bei filled with mercury, and placed inverted into a basin of me cury, if we let up into it about 19 grains of water, and rai the temperature to 212° the water will disappear, being co verted into vapour, and the mercury will at the same time si to the bottom of the jar, the 19 grains of water converted in vapour occupying the volume of 100 cubic inches. The bu into which water expands by the ordinary boiling heat, pr ducing a steam equally elastic with common air, is very gre But it was very much overrated at first. Dr Desaguli reckoned the expansion 14,000 times the original bulk. Watt was the first person who determined it with an approa to accuracy. From his experiments he drew, as a consequen that water when converted into steam increases in bulk 18 times. From the experiments of Guy-Lussac it appears th the specific gravity of steam is 0·625, that of air being 1. therefore, 100 cubic inches of air weigh 31·1446 grains, follows from the experiments of Biot and Arago, it is obvi that the same volume of steam will weigh 19·465 grai Now, a cubic inch of water at 60° weighs 252·52 grains. is obvious that 252·52 grains of water, when converted i steam, would occupy 1297·3 cubic inches, which, therefo should be the true increase of volume when water is conver into steam. But if 0·625 be the specific gravity of steam

Dilatation of water when converted into steam.

212° compared with that of air, reckoned 1 at the same temperature, then the expansion will be greater, and will approach nearly to the determination of Mr Watt. For on that supposition the specific gravity of steam at 212° compared with air, reckoned 1 at 60° would be only 0·481. So that 100 cubic inches of it would weigh 14·98 grains; and a cubic inch of water, when converted into steam, would expand into 1689 cubic inches. Sect. I.

This change, like fluidity, is produced by the increase of the temperature of the body so changed. Now the temperature at which the change takes place is found to differ in almost every kind of matter. The temperature must be kept up, in order that the vaporous form may remain. If the heat be reduced the vapour loses its elasticity, and collapses into that form from which it was produced. Thus steam, whenever it impinges against a cold body, is immediately condensed again into water.

Volatile and fixed bodies.

2. The *vaporific* or *boiling* point of most bodies is higher than that which is necessary to give them liquidity; but to this there are some exceptions. Thus arsenic and benzoic acid are converted into vapours at a lower temperature than is required to melt them. Bodies easily converted into vapour, are called *volatile;* while those are called *fixed*, which require a high temperature to induce this change, or which cannot be converted into vapour, by the greatest heat which we have it in our power to raise.

Boiling point of liquids.

3. The temperature at which bodies are converted into vapour is usually called their *boiling point*. This point is very different in different liquids; but, other things remaining the same, it is constant for every particular liquid. The following table exhibits the boiling points of a few liquids, in which that point has been determined with precision:—

	Boiling Point.
Muriatic ether	52°
Sulphuric ether	96
Bisulphuret of carbon	116
Ammonia (sp. gr. 0·945)	140
Oil of laurel*	150

* This is a volatile oil which comes from Demerara. It is obtained by

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	Boiling point.
Acetic ether	160°
Alcohol (sp. gr. 0·798)	173
Water	212
Nitric acid (sp. gr. 1·42)	248
Crystallized muriate of lime	302
Oil of turpentine	314
Naphtha	320
Phosphorus	554
Sulphuric acid (sp. gr. 1·843)	620
Whale oil	630
Mercury	668

The boiling points of the following saline solutions ha[ve] been determined by Mr Griffith :—*

Of saline solutions.

	Salt in 100 of the solution.	Boiling point.
Sulphate of soda	31·5	213°
Corrosive sublimate	——	214
Cyanodide of mercury	3·5	214
Acetate of copper	16·5	214
Bitartrate of potash	9·5	214
Nitrate of barytes	26·5	214
Sulphate of potash	17·5	215
Acetate of lead	41·5	215
Nitrate of lead	52·5	216
Protosulphate of iron	64	216
Sulphate of copper	45	216
Potash-sulphate of copper	40	217
Boracic acid	—	218
Chlorate of potash	40	218
Prussiate of potash	55	218
Oxalate of ammonia	29	218
Oxalate of potash	40	220
Alum	52	220
Sulphate of zinc	45	220

tapping some unknown tree in the interior of South America. It i[s a] volatile oil holding a resin in solution, and begins to boil, I find, at 15[0]. But the temperature rapidly rises as the boiling proceeds; showing t[he] presence of a number of oils differing in their volatility.

* Journal of Science, xviii. 90.

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	Salt in 100 of the solution	Boiling point.
Chloride of barium	45	220°
Carbonate of soda	—	220
Phosphate of soda	—	222
Borax	52·5	222
Bisulphate of potash	——	220
Sulphate of magnesia	57·5	222
Nitrate of strontian	53	224
Common salt	30	224
Tartrate of potash	68	234
Sulphate of nickel	65	235
Sal ammoniac	50	236
Nitrate of potash	74	238
Potash tartrate of soda	90	240
Nitrate of soda	60	246
Acetate of soda	60	256
Chloride of calcium (saturated)		264*

Boiling point affected by the height of the barometer.

The boiling point of liquids is not quite so steady as the g or freezing point; for it is affected by the degree of ıre to which the liquid is exposed. This was first ob- l by Boyle while engaged in his experiments with the mp. Fahrenheit afterwards noticed that the boiling of water, examined by the same thermometer, differs at nt times. And he found that this depended upon the of the barometer. When the barometer stands high iling point is a little higher than usual, while it is a little than usual when the barometer is low. The knowledge s fact induced him to attend to the height of the baro- when he graduated his thermometers. The determi- of General Roy has been generally adopted in this y, as upon the whole the most correct. The following shows the temperature at which water boils (according ı) at the different heights of the barometer:—

Barometer.	Boiling point.
26 inches	204°·91
26·5	205·79
27	206·67

* By my determination.

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Barometer.	Boiling point.
27·5	207°·55
28	208·43
28·5	209·31
29	210·19
29·5	211·07
30	212
30·5	212·88
31	213·76

From this table it appears that for every half inch of variation in the barometer, the boiling point of water varies 0·88 of a degree. Consequently every tenth of an inch that the barometer rises or falls either raises or lowers the boiling point of water by 0·176 of a degree.

The mean height of the barometer at the sea-shore is 29·82 inches. In this country it seldom rises much higher than 31 inches, or sinks much lower than 28½ inches. Since the year 1817 to 1839 (both inclusive) it has never been higher in Glasgow (at about 50 feet above the level of the sea) than 30·8 inches, nor lower than 27·95.* So that in Glasgow the boiling point of water varies from 213°¾ to 208°·4, or almost 5·33 degrees of Fahrenheit.

Elasticity of steam increased by heat in close vessels.

5. When water is heated in close vessels and cannot make its escape till it overcome a considerable pressure, its boiling point may be made to rise to a much higher temperature than 212°. Papin, an ingenious French physician, who resided in London, contrived a vessel for this purpose, to which he gave the name of *digester*. It was a cylindrical copper vessel, having a lid nicely fitted to it and kept fast by screws. If this vessel be half filled with water, and the lid screwed down tight, and if it be then set upon burning coals, a portion of the water is soon converted into *steam*. This conversion begins at the boiling point of water. But the elastic vapour being confined, presses upon the surface of the water, and thus prevents the conversion of any more of it into steam, till the temperature of the water rises above the boiling point. This heat being conveyed to the steam, it now becomes capable of bear-

* It was as low as 27·95 inches on Thursday, the 29th November, 18[illegible]

ing the addition of another portion of vapour without being condensed into water. Thus the quantity, and consequently the elasticity of the steam is continually increasing with the temperature of the water. By this contrivance, the temperature of water has been raised, in close vessels, as high as 419°. Muschenbroek even assures us that he raised the temperature of water, in digesters, high enough to melt tin. Now, tin fuses at the temperature of 442°. Sect. I.

The elasticity of steam at 419°, is 1050 times greater than that of atmospheric air; so that it exerts a force equivalent to 14,700 lbs. upon every square inch of the inside of the vessel in which it is confined. This pressure is so enormous, that few vessels can be made strong enough to withstand it. Dreadful accidents have taken place in consequence of the bursting of such vessels. It was to guard against the possibility of these, that what is known by the name of the *safety valve* was added to the digester. This consists of an opening made in the digester to let out the steam when it becomes too elastic to be confined, without hazarding the bursting of the vessel. This opening is covered with a metal plate, ground flat so as to fit it exactly. This mouth piece is kept down by a lever, to the extremity of which is fixed a weight. This weight is so contrived as to exert a force capable of being overcome before the elasticity of the steam be great enough to burst the vessel. Suppose the surface of the safety valve to be a square inch: if the weight exert an action equivalent to 15 lbs. the elasticity of the steam must be double that of the atmosphere, or its temperature must be 250° before it forces open the safety valve.

Safety valve.

6. When the steam of boiling water comes in contact with any part of the living body, it occasions instantly a most severe scald; but when steam from water of a higher temperature than boiling water, *high pressure steam*, as it is called, issues into the atmosphere, the finger or any part of the body may be passed through it with impunity. It has not the property of scalding; and if a thermometer be put into it, we find the temperature greatly below that of boiling water; so that high pressure steam has a much *lower* temperature

Why high pressure steam does not scald.

 than *low* pressure steam, or steam issuing freely from boiling water.

Whoever has an opportunity of seeing these two different species of steam, will find no difficulty in understanding the reason of this difference. When steam issues from the spout of a boiling tea-kettle, it is at first invisible, and it is not till it has advanced some distance in the air, that it begins to assume the appearance of a visible cloud. But condensed steam is visible the instant that it issues from the mouth of the pipe. The high pressure steam (supposing its elasticity double) occupies only half the bulk of common steam. The moment it comes into the atmosphere its volume is doubled. This occasions a prodigious increase in its capacity for heat, and at the same time mixes it with the cold atmospheric air. These two circumstances sink its temperature so low, that it is no longer capable of scalding.*

Boiling point in vacuo.

When the pressure of the atmosphere is withdrawn, the temperature at which liquids boil becomes much lower. From the experiments of the late Dr John Robison, of Edinburgh, it appears that by removing the pressure of the atmosphere, the boiling point of liquids is lowered about 145° : hence, in a good vacuum, sulphuric ether will boil at the temperature of —49°, alcohol will boil at 28°, water at 67°, and sulphuric acid at 460°. Mr Barry, of London, has applied his knowledge of this fact to the preparation of volatile oils, and the various inspissated juices of plants used in medicine. The still is connected with a globular copper receiver of large size, which is filled with steam to the exclusion of the atmospheric air. This steam being condensed and the communication between the still and receiver being opened, the air in the still divides itself equally over the still and receiver. The receiver being 5 times the size of the still, it is obvious that by this first condensation of

* It is obvious that when high pressure steam is conveyed in pipes, it will undergo an expansion or condensation in proportion as the diameter of the pipe increases or diminishes. Accordingly, its temperature will be found to vary according to the diameter of the pipe. Upon this subject, the reader may consult a very curious set of experiments by Peter Ewart, Esq., in the Philosophical Magazine (new series), vol. v. p. 247.

steam, the density of the air within the apparatus is reduced to $\frac{1}{5}$. Another repetition of the same process will reduce the density of the air within the apparatus to $\frac{1}{25}$, and a third to $\frac{1}{125}$. And it is obvious, that by continuing the repetitions long enough, any degree of exhaustion whatever might be obtained. I have seen water boiling briskly in Mr Barry's apparatus at the temperature of 98°.

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Refining sugar.

This method of boiling in vacuo was applied very ingeniously by Mr Edward Howard, to the refining of sugar. The syrup is thus concentrated to the granulating point without any risk of burning or decomposing any of the sugar by too much heat. This process is now very generally followed by the sugar refiners of Great Britain. The sugar refined in this way is beautifully white, and the loss sustained during the process is greatly diminished.

Steam engine.

As steam possesses the elasticity of air, and as it may be immediately condensed by the application of cold, it is obvious that it may be immediately applied as a moving force, and that it possesses in this way almost unlimited power. The medium in which it is so applied, is familiarly known by the name of steam *engine*, and constitutes the finest present ever made by science to the arts. It is admitted to have been invented by the Marquis of Worcester; though it does not appear that he was ever able to interest the public in his invention, or that he attempted to apply it to any useful purpose.

It was reinvented by Captain Savery, who took out a patent towards the end of the 17th century, and published an account of it in the year 1696, in a book entitled the *Miner's Friend.* In Savery's machine, the elasticity of steam was applied directly to force water up a pipe. The waste of steam was so enormous, and the quantity of fuel necessary so great, that it does not appear ever to have been attempted to apply it directly for the purpose of draining water out of mines, which was the object that Savery had in view when he took out his patent.

In 1705, a new patent was taken out jointly by Captain Savery, Mr Newcomen, a blacksmith at Dartmouth, in Devonshire, and Mr Crawley, a glazier, in the same place. The

Chap. III. merit of the machine has been universally ascribed to Newcomen, under whose name it always went. It consisted essentially of a metallic cylinder, in which a piston, made air tight, was capable of moving from the top to the bottom. The top of the cylinder was open, the bottom close. The piston was attached to the piston rod or chain, which connected it with the end of the working beam. The working beam was supported on a gudgeon, and the end opposite to that to which the piston rod was attached was loaded, and of course the pump rod attached to it, was at the bottom of the well from which the water was to be pumped. The cylinder is filled with steam, till all the air is driven out. The piston was at the top of the cylinder. The steam in the cylinder is condensed by means of a jet of cold water. A vacuum is produced in the cylinder. The atmosphere presses upon the top of the piston, and forces it to the bottom of the cylinder. The pump rod at the other end of the working beam is drawn up. It makes a stroke, and a quantity of water is pumped out of the well or mine. Steam is again introduced below the piston in the cylinder, the vacuum is removed and the piston rises to the top in consequence of the load at the other extremity of the working beam. The cylinder is filled with steam as before; this steam is condensed, the piston is forced down, more water is pumped up, and thus the machine continues to act as long as it is supplied with steam.

The great improvement in the steam engine was made by Mr Watt. He contrived to condense the steam in a separate pipe, and by this means prevented the cylinder from being cooled by the injection of cold water. This produced an enormous saving of fuel. He substituted steam for the pressure of the atmosphere in Newcomen's engine, and made the machine to act both when the piston descended and ascended the cylinder. This alteration, together with the mode of producing a circular motion, which he contrived, enabled him to apply the steam engine as a power for moving machinery. It is now used in Great Britain to a prodigious extent, and has carried the manufactures of the country to a pitch which it was impossible to have anticipated. It has of late been

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applied to move vessels in the water, independent of wind or tide, and even against those powers on which formerly the motions of ships entirely depended. Thus it is likely to introduce as great an alteration by sea as it has done by land. But it is not our province to enter upon any description of this wonderful machine. Abundance of descriptions of it have been laid before the public, to which we beg leave to refer the reader.

Elasticity of vapour below 212°.

7. Water is converted into vapour at all temperatures, even at 32° or lower. But the elasticity at low temperatures is low, and it increases as the temperature increases, till at 212° it is equal to that of the atmosphere, or capable of supporting a column of mercury 30 inches in height.

As steam is employed in the steam engine as a moving force, it becomes a question of considerable importance to determine its elasticity at different temperatures, because the knowledge of this will put it in the power of engineers to determine the temperature at which it may be most economically employed, and the strength of the vessels which will be necessary when steam is used at elevated temperatures. The first set of experiments on this subject, approaching to accuracy, was made by Professor John Robison, of Edinburgh, and inserted by him in the article *Steam*, in the third edition of the *Encyclopædia Britannica*, published about the year 1797. Mr Dalton published a still more accurate set of experiments in 1802.*

He determined the elasticity of steam from 32° to 212° by experiment; but the elasticities below 32° and above 212° were inferred from observing the rate at which the elasticities determined by experiment increased. Unfortunately the application of empirical formulæ never gives results which can be depended on, unless they be confined within the limits of the experiments. Dr Ure, of Glasgow, made an elaborate set of experiments on the same subject in 1816, the result of which is published in the *Philosophical Transactions* for 1818.† Mr Southern had made a set of experiments on the same subject in 1803, and carried them to considerably higher tempera-

* Manchester Memoirs, v. 559. † Page 338.

Chap. III. tures than Dr Ure had it in his power to do. These experiments have been given to the public by Mr Watt.* They seem to have been made with great care, and are therefore of considerable importance.

The results of all these experimenters may be seen in the following table :—

Table of the elasticity of steam at different temperatures.

I.—*Elasticity of Vapour below 32° according to Dalton.*

Temperature.	Force of vap. in inches of mercury.	Temperature.	Force of vap. in inches of mercury.	Temperature.	Force of vap. in inches of mercury.	Temperature.	Force of vap. in inches of mercury.
—40°	0·013	5°	0·076	14°	0·104	23°	0·144
—30	0·020	6	0·079	15	[illegible]	24	0·150
—20	0·030	7	0·082	16	0·112	25	0·156
—10	0·043	8	0·085	17	0·116	26	0·162
0	0·064	9	0·087	18	0·120	27	0·168
1	0·066	10	0·090	19	0·124	28	0·174
2	0·068	11	0·093	20	0·129	[illegible]	0·180
3	0·071	12	0·096	21	0·134	30	0·186
4	0·074	13	0·100	22	0·139	31	0·193

Temp.	Force of vap. in inches of mercury.				Temp.	Force of vap. in inches of mercury.			
	Robison.	Dalton.	Ure.	Southern.		Robison.	Dalton.	Ure.	Southern.
32°	0·0	0·200	0·200	0·16	56°		0·458		
33		0·207			57		0·474		
34		0·214			58		0·490		
35		0·221			59		0·507		
36		0·229			60	0·35	0·524	0·516	
37		0·237			61		0·542		
38		0·245			62		0·560		0·52
39		0·254			63		0·578		
40	0·1	0·263	0·250		64		0·597		
41		0·273			65		0·616	0·630	
42		0·283		0·23	66		0·635		
43		0·294			67		0·655		
44		0·305			68		0·676		
45		0·316			69		0·698		
46		0·328			70	0·65	0·721	0·726	
47		0·339			71		0·745		
48		0·351			72		0·770		0·73
49		0·363			73		0·796		
50	0·2	0·375	0·360		74		0·823		
51		0·388			75		0·851	0·860	
52		0·401		0·35	76		0·880		
53		0·415			77		0·910		
54		0·429			78		0·940		
55		0·443	0·416		79		0·971		

* They constitute an Appendix to Mr Watt's edition of Professor Robison's articles *Steam* and *Steam Engines*.

TABLE *continued.*

Temp.	Force of vap. in inches of mercury.				Temp.	Force of vap. in inches of mercury.			
	Robison.	Dalton.	Ure.	Southern.		Robison.	Dalton.	Ure.	Southern.
80°	0·82	1·00	1·010		138°		5·44		
81		1·04			139		5·59		
82		1·07		1·02	140	5·15	5·74	5·770	
83		1·10			141		5·90		
84		1·14			142		6·05		6·10
85		1·17	1·170		143		6·21		
86		1·21			144		6·37		
87		1·24			145		6·53	6·600	
88		1·28			146		6·70		
89		1·32			147		6·87		
90	1·18	1·36	1·360		148		7·05		
91		1·40			149		7·23		
[illegible]		1·44		1·42	150	6·72	7·42	7·530	
[illegible]		1·48			151		7·61		
94		1·53			152		7·81		7·90
95		1·58	1·640		153		8·01		
96		1·63			154		8·20		
97		1·68			155		8·40	8·500	
98		1·74			156		8·60		
99		1·80			157		8·81		
100	1·6	1·86	1·860		158		9·02		
101		1·92			159		9·24		
102		1·98		1·96	160	8·65	9·46	9·600	
103		2·04			161		9·68		
104		2·11			162		9·91		10·05
105		2·18	2·100		163		10·15		
106		2·25			164		10·41		
107		2·32			165		10·68	10·800	
108		2·39			166		10·96		
109		2·46			167		11·25		
110	2·25	2·53	2·456		168		11·54		
111		2·60			169		11·83		
112		2·68		2·66	170	11·05	12·13	12·050	
113		2·76			171		12·43		
114		2·84			172		12·73		12·72
115		2·92	2·810		173		13·02		
116		3·00			174		13·32		
117		3·08			175		13·62	13·550	
118		3·16			176		13·92		
119		3·25			177		14·22		
120	3·0	3·33	3·300		178		14·52		
121		3·42			179		14·83		
122		3·50		3·58	180	14·05	15·15	15·160	
123		3·59			181		15·50		
124		3·69			182		15·86		16·01
125		3·79	3·830		183		16·23		
126		3·89			184		16·61		
127		4·00			185		17·00	16·900	
128		4·11			186		17·40		
129		4·22			187		17·80		
130	3·95	4·34	4·366		188		18·20		
131		4·47			189		18·60		
132		4·60		4·71	190	17·85	19·00	19·000	
133		4·73			191		19·42		
134		4·86			192		19·86		20·04
135		5·00	5·070		193		20·32		
136		5·14			194		20·77		
137		5·29			195		21·22	21·100	

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TABLE *continued*.

Temp.	Force of vap. in inches of mercury. Robison	Dalton	Ure	Southern	Temp.	Force of vap. in inches of mer[cury] Robison	Dalton	Ure	S[outhern]
196°		21·68			234°·5			46·800	
197		22·13			235		45·58	47·220	
198		22·69			236		46·39		
199		23·16			237		47·20		
200	22·62	23·64	23·600		238		48·12		
201		24·12			238·5			50·40	
202		24·61		24·61	239		48·84		
203		25·10			240	54·9	49·67	51·70	
204		25·61			242			53·60	
205		26·13	25·900		245		53·88	56·34	
206		26·66			248·5			60·40	
207		27·20			250	66·8	56·21	61·90	60
208		27·74			251·6				
209		28·29			255		62·85	67·25	
210	28·65	28·84	28·880		260	80·3	67·73	72·30	
211		29·41			264·2				
212		30·00	30·000	30·00	265		72·76	78·04	
213		30·60			270	94·1	77·85	86·30	
214		31·21			275		83·13	93·48	
215		31·83			280	105·9	88·75	101·90	
216		32·46			285		94·35		
216·6			33·40		288·2			112·20	
217		33·09			290		100·12	120·15	
218		33·72			291·4				120
219		34·35			295		106·97	129·00	
220	35·8	34·99	35·540		300		111·81	139·70	
221		35·63			302				
221·6			36·700		305		117·68	150·56	
222		36·25			309·2				
223		36·88			310		123·53	161·30	
224		37·53			312			166·25	
225		38·20	39·110		316·4				
226		38·88			320		135·40		
226·3			40·100		322·7				
227		39·56			326·46				
228		40·30			330				
229		41·03			334·4				
230	44·5	41·75	43·100		339·26				
230·5			43·500		340				
231		42·49			343·4				
232		43·24			345·6				240
233		44·00			419				
234		44·78							

But by far the most extensive experiments on this s[ubject] were made by a committee of the French Academy of Sci[ences] consisting of Prony, Arago, Gerard, and Dulong. [These] experiments were the result of an application of the F[rench] government to the Academy, to point out the best mea[ns of] preventing accidents from the bursting of the boilers of [steam] engines. The following table exhibits the elasticity of [steam] at various temperatures, till it amounts to 24 atmospher[es]

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ermined by these gentlemen: an atmosphere is measured a column of mercury of 29·922 inches (0·76 metre), which been adopted in France as the mean height of the barometer at the surface of the sea:—

Elasticity of steam, the pressure of the atmosphere being 1.	Corresponding temp. in deg. of Fahrenheit.	Elasticity of steam, the pressure of the atmosphere being 1.	Corresponding temp. in deg. of Fahrenheit.
1	212°	13	380·66°
$1\frac{1}{2}$	234	14	386·94
2	250·5	15	392·86
$2\frac{1}{2}$	263·8	16	398·48
3	275·2	17	403·83
$3\frac{1}{2}$	285	18	408·92
4	293·7	19	413·78
$4\frac{1}{2}$	300·3	20	418·46
5	307·5	21	422·96
$5\frac{1}{2}$	314·24	22	427·28
6	320·36	23	431·42
$6\frac{1}{2}$	326·26	24	435·56
7	331·7		
$7\frac{1}{2}$	336·86	25	439·34
8	341·78	30	457·16
9	350·78	35	472·73
10	358·88	40	486·59
11	366·85	45	499·14
12	374	50	510·6

he last six temperatures in the table are deduced by calion. The Commissioners of the Academy employed the ula $e = (+0\cdot7153\ t)^5$. In this formula e expresses the icity in atmospheres of 0·76 metre (29·922 inches) prese, and t the temperatures setting out at 100° centigrade 2° Fahrenheit). For the methods employed to determine temperature at the various elasticities in the table, and the autions taken to avoid errors, I refer the reader to the port inserted in the *Annales de Chimie et de Physique*, ii. 74.

This table of the French Academicians constitutes a preus addition to our knowledge of the elasticity of steam. e experiments which were conducted by Dulong and Arago, stitute some of the most difficult and even dangerous for ch science is indebted to the zeal and intrepidity of philobers. Mercury, by the elasticity of the steam, was forced

Chap. III. up a tube of glass to the height of 42·6517 feet. The diameter of the tube was 0·1968 inches. They took the opportunity of subjecting common air to the pressure of from 1 to 24 atmospheres, and found that the Boylean law, namely, that the bulk diminishes as the pressure increases, holds as far as twenty-four atmospheres.

We see that the elasticity increases at a very rapid rate with the temperature. Attempts have been made to represent this increase by empirical formulas, so as to enable us to calculate the elasticity of steam for any given temperature. But such formulas, from the imperfection of the data on which they are founded, cannot be accurate, and indeed do not apply to higher temperatures than those deduced from experiment. They are therefore of little use; and indeed are very apt to mislead. I think it better therefore to omit them.

8. It will be seen by inspecting the table inserted in page 178 of this volume, that the boiling point of saline solutions is higher than that of water. Thus, a saturated solution of saltpetre does not boil till raised to the temperature of 238°. Now, it has hitherto been supposed, that the steam emitted from these boiling solutions is as high as the boiling point of the solution itself. But Rudberg has shown that this is not the case: and that whatever be the strength of the boiling solution, or the temperature at which it boils, the heat of the steam, *ceteris paribus*, is always 212°.* The saline solutions tried were of nitrate of lime of various strengths, of saltpetre, of carbonate of potash, and of chloride of calcium.

The reason of this constancy at 212° is pretty obvious; and might have been foreseen had those who affirmed the contrary taken the trouble to consider the phenomena with attention. Suppose we dissolve 74 parts of saltpetre in 100 parts of water, which is nearly the solubility of that salt in water of the temperature 212°. The solution consists of an atom of saltpetre united to about 16 atoms of water. The water must be attracted by the salt and thus prevented from escaping in steam till the temperature reaches 238°, when it is high

* Poggendorff's Annalen, xxxiv. 257

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enough to have an elasticity capable of balancing that attraction. But only pure water is converted into steam, for no sensible quantity of the salt is evaporated. The instant that this steam quits the solution, its temperature must be 238°. But as its density is greater than that of the atmosphere, it expands the moment it assumes the elastic form till its elasticity just equals that of the atmosphere. This expansion, which is instantaneous, reduces its temperature to 212°. Whatever therefore may be the heat of boiling saline solution, that of the steam measured by the thermometer must be always 212°.

Vapours from other liquids.

9. Water is not the only liquid whose vapour has been examined with care. There are eight others whose boiling points, and the specific gravity of whose vapours have been determined almost entirely by the sagacity and industry of M. Gay-Lussac. The following table exhibits the boiling points and specific gravities of the vapours of these various liquids:—

	Sp. gravity, air being 1.	Boiling point.
Water	0·6235*	212°
	0·6250†	
Hydrocyanic vapour	0·9476*	79·7
Pyroxylic spirit	1·120	150
Alcohol	1·6030*	173
Muriatic ether	2·219‡	52
Sulphuric ether	2·586*	96
	2·5808†	
Bisulphuret of carbon	2·6447*	116
	2·6386†	
Oil of turpentine	5·013*	314
Hydriodic ether	5·4749*	148

10. These specific gravities are compared to that of air, which is considered as 1, when heated to the boiling point of each liquid. They do not therefore enable us to form a correct idea of the increase of volume, which takes place when each

* Gay-Lussac, Ann. de Chim. xci. 95, 150.—Ann. de Chim. et de Phys. i. 218.

† Despretz, Ann. de Chim. et de Phys. xxi. 143.

‡ Thenard, Mem. d'Arcueil, i. 121.

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liquid is converted into vapour. I have therefore calcul[ated] the following table, exhibiting the specific gravity of the va[pour] of each at the boiling point, if we reckon the specific gra[vity] of air at 60° to be unity :—

	Sp. gravity at boiling point, air at 60° being 1.
Steam	0·481
Hydrocyanic vapour	0·912
Alcohol	1·311
Muriatic ether	2·255
Sulphuric ether	2·415
Bisulphuret of carbon	2·376
Oil of turpentine	3·342
Hydriodic ether	4·666

The specific gravity of these liquids, from which th[e] vapours are produced, is at the temperature of 60° as [fol]lows :—

True specific gravity of.

	Sp. gravity.
Water	1·000
Hydrocyanic acid	0·7039
Alcohol	0·798
Muriatic ether	0·874*
Sulphuric ether	0·632
Bisulphuret of carbon	1·272
Oil of turpentine	0·792
Hydriodic ether	1·9206†

By a very careful set of experiments made by Dr Pr[out] it has been ascertained that at the temperature of 32°, [and] under a pressure of 30 inches mercury, 100 cubic inches [of] atmospherical air weigh 32·79 grains; and at the tempe[ra]ture of 60°, and when the barometer stands at 30 inches, [100] cubic inches of air weigh 31·0117 grains. From these [data] it is easy to calculate the increase of bulk which each of th[ese] liquids undergoes when converted into vapour, at the boil[ing] point of each liquid :—

Expansion of liquids when converted into vapour.

	Increase of volume when converted into vapour	Increase of volume, that of turpentine vapour being 1
Water	1689	8·79
Hydrocyanic acid	625·7	3·25
Alcohol	493·5	2·57

* At 41°. † At 72°.

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	Increase of volume when converted into vapour.	Increase of volume, that of turpentine vapour being 1.
Muriatic ether .	314·15	. . 1·6
Sulphuric ether .	212·18	. 1·1
Bisulphuret of carbon	434·06	. . 2·26
Oil of turpentine	192·15	. . 1
Hydriodic ether .	333·74	. . 1·7*

It is obvious from this table that when water is converted into steam, it undergoes a much greater expansion than any other liquid hitherto examined. It expands 8 times as much as sulphuric ether, and almost $3\frac{1}{2}$ times as much as alcohol. This explains a phenomenon, which may at first sight appear paradoxical,—why the vapours of alcohol and ether are heavier than those of water, though the liquids themselves which produce these vapours are lighter than water.

11. I had supposed from analogy that the specific gravity of the vapour of water is proportional to its elasticity. Had this been so, as we know the specific gravity of steam at 212° to be 0·625, it would be easy to calculate it for any other temperature; but M. Despretz has shown by experiment, that this analogy does not hold.† An empirical formula might be deduced from Despretz's experiments, provided we could confide in their accuracy; but it will be sufficient, if in this place I give the weight of 100 cubic inches of vapour, at

* Cagnard de la Tour made a curious set of experiments which deserve to be noticed. He enclosed ether, alcohol, and water, in small glass tubes, which were never more than half filled. He sealed the tubes hermetically without expelling the air from the empty portion. He then exposed the tubes to heat till the liquids assumed the gaseous form.

Ether became gaseous in a space scarcely double its volume at the temperature of 320°: pressure 38 atmospheres. Alcohol became gaseous in a space about thrice its volume at the temperature of 404°$\frac{1}{2}$: pressure about 129 atmospheres. Water broke the glass tube, because it began to dissolve it. When this was prevented by adding a little carbonate of soda, the water became gaseous in a space four times its volume at the temperature at which zinc melts, or about 773°.

† Ann. de Chim. et de Phys. xxi. 150. He found the specific gravity of vapour to be at

66°·75	0·00772
63·39	0·00587

Chap. III. all temperatures from 32° to + 80° as calculated by M Dalton :—*

Temp.	Weight of 100 inches. Grains.	Temp.	Weight of 100 inches. Grains.	Temp.	Weight of 100 inches. Grains.
32°	0·178	49°	0·313	66°	0·492
33	0·184	50	0·323	67	0·509
34	0·191	51	0·329	68	0·521
35	0·197	52	0·341	69	0·539
36	0·203	53	0·354	70	0·551
37	0·209	54	0·366	71	0·569
38	0·216	55	0·378	72	0·580
39	0·222	56	0·384	73	0·598
40	0·229	57	0·396	74	0·610
41	0·235	58	0·402	75	0·627
42	0·245	59	0·414	76	0·645
43	0·255	60	0·420	77	0·662
44	0·267	61	0·432	78	0·680
45	0·275	62	0·444	79	0·700
46	0·284	63	0·456	80	0·721
47	0·293	64	0·468		
48	0·303	65	0·480		

Theory of steam.

12. Such are the phenomena of the conversion of liquid into elastic fluids. Dr Black applied his theory of latent heat to this conversion with great sagacity, and demonstrate that it is owing to the very same cause as the conversion of solids into liquids; namely, to the combination of a certai dose of caloric with the liquid, without any increase of tem perature. The truth of this very important point was esta blished by the following experiments :—

First. When a vessel of water is put upon the fire, th water gradually becomes hotter till it reaches 212°; after wards its temperature is not increased. Now, heat must b constantly entering from the fire and combining with th water. But as the water does not become hotter, the hea must combine with that part of it which flies off in the forn of steam: but the temperature of the steam is only 212° therefore this additional heat does not increase its tempera ture. We must conclude, then, that the change of water t steam is owing to the combination of this heat; for it pro duces no other change.

Dr Black put some water in a tin-plate vessel upon a re

* New System of Chemistry, ii. 299.

hot iron. The water was of the temperature 50°: in four minutes it began to boil, and in twenty minutes it was all boiled off. During the first four minutes it had received 162°, or 40½° per minute. If we suppose that it received as much per minute during the whole process of boiling, the caloric which entered into the water and converted it into steam would amount to $40\frac{1}{2} \times 20 = 810$.* This heat is not indicated by the thermometer, for the temperature of steam is only 212°; therefore Dr Black called it *latent heat.*

Second. Water may be heated in a Papin's digester to 400° without boiling: because the steam is forcibly compressed, and prevented from making its escape. If the mouth of the vessel be suddenly opened while things are in this state, part of the water rushes out in the form of steam, but the greater part still remains in the form of water, and its temperature instantly sinks to 212°; consequently 188° of heat have suddenly disappeared. This heat must have been carried off by the steam. Now, as only about ⅕th of the water is converted into steam, that steam must contain not only its own 188°, but also the 188° lost by each of the other four parts; that is to say, it must contain 188° × 5, or about 940°. Steam, therefore, is water combined with at least 940° of heat, the presence of which is not indicated by the thermometer. This experiment was first made by Dr Black, and afterwards, with more precision, by Mr Watt.

Third. When hot liquids are put under the receiver of an air-pump, and the air is suddenly drawn off, the liquids boil, and their temperature sinks with great rapidity a considerable number of degrees. Thus water, however hot at first, is very soon reduced to the temperature of 70°; and ether becomes suddenly so cold that it freezes water placed round the vessel which contains it. In these cases the vapour undoubtedly carries off the heat of the liquid; but the temperature of the vapour is never greater than that of the liquid itself: the heat therefore must combine with the vapour, and become latent.

Fourth. If one part of steam at 212° be mixed with nine parts by weight of water at 62°, the steam instantly assumes

* Black's Lectures, i. 157.

Chap. III. the form of water, and the temperature after mixture is 178°·6; consequently each of the nine parts of water has received 116°·6 of caloric; and the steam has lost 9 × 116·6 = 1049°·4 of caloric. But as the temperature of the steam is diminished by 33°·3, we must subtract this sum. There will remain rather more than 1000°, which is the quantity of heat which existed in the steam without increasing its temperature. This experiment cannot be made directly, but it may be made by passing a given weight of steam through a metallic worm, surrounded by a given weight of water. The heat acquired by the water indicates the heat which the steam gives out during its condensation. Mr Watt was the first person who attempted to determine accurately the latent heat of steams with precision. His experiments were made in 1781; but the account of them has been only published in Mr Watt's edition of the articles *Steam* and *Steam Engine*, by Professor Robison.* The result of the experiments is, that the latent heat of steam is 950°. This agrees nearly with the determination of Mr Southern in 1803. He found it in three experiments made at very high temperatures 942°, 942°, and 950°.† The experiments of Lavoisier make it rather more than 1000°. According to Rumford it amounts to 1040°·8. Thus we have the latent heats of steam as follows:—

Watt	. . . . .	950
Southern	. . . .	945
Lavoisier	. . . .	1000
Rumford	. . . .	1040·8
Despretz	. . . .	955·8
	Mean . .	978·3

This number 978°·3 cannot deviate very far from the truth. We cannot err much then, if we adopt for the latent heat of steam, 1000°, which was the number established by the experiments of Lavoisier. I am satisfied from some trials which I some time ago made, that the true number is not under 1000°, if it does not exceed it.

Latent heat increases as the sensible diminishes.

13. It has been already observed, that in a vacuum, the boiling point of liquids is lowered by about 145°. It occurred

* Page 6. † Ibid. p. 165.

to Dr Black many years ago, that by distilling ardent spirits in vacuo, a great saving of fuel would probably result. The project was first attempted to be realised by Mr Watt. He half filled a small still with water, and then closely united it with the receiver. The receiver had a small hole in its bottom, which had a plug fitted to it. The water in the still was made to boil violently till all the air within the apparatus was forced by the steam out at this hole. While the steam was blowing violently out at the hole, it was suddenly stopped by the plug, and the bottom of the still was set on ice. This soon cooled the contents, and the steam within the apparatus was condensed into water. A lamp was now set under the still, and in a few minutes, the whole apparatus grew warm—a proof that steam was produced from the water, and that it was passing into the receiver. Cold water was put into the refrigeratory and the distillation went on—slowly indeed, but very well; and the ebullition was distinctly heard in the still although the head of it was scarcely sensibly warm to the hand: but the result of the process was, that the latent heat of the steam was greatly increased by the diminution of its sensible heat. The temperature of the steam in the experiment was found to be 100°. The water in the refrigeratory was raised by the condensation of this steam from 57° to 77°, and the vessel had acquired as much heat as would have raised the water 1°; consequently 21° of heat had been acquired from the steam. The quantity of water distilled was $\frac{1}{51}$ of that in the refrigeratory. Therefore, 21° × 51 will give the heat extricated from the steam. This amounts to 1071. From this we must subtract the sensible heat lost by the steam; for it was cooled down from 100° to 77°, so that it lost 23°. The remainder 1048°, is the latent heat of the steam. The latent heat of steam at 212°, according to Mr Watt's experiments, made with a similar apparatus, is 940°: hence it follows, that the latent heat of steam of 100° exceeds that of 212° by 108°, which is very nearly the difference between the sensible heats of the two steams.

Mr Watt made other experiments with much more care. He found that water distils perfectly well at 70°, and that in

Chap. III. this state the latent heat approaches 1300°, and certainly exceeds 1200°. The unexpected result of these experiments is, that no advantage is to be expected in the manufacture of ardent spirits by distilling in vacuo; for the latent heat of the steam is as much increased as the sensible heat is diminished.

A little consideration will satisfy us that this fact might have been anticipated, had Mr Watt, when he made his experiments, been aware that the volume of vapour in a given weight increases in proportion as its sensible heat diminishes; for its specific heat must increase with this augmentation of volume, and occasion an increase of heat necessary to maintain its temperature just proportional to the diminution of the sensible heat.

And vice versa.

In the year 1813, a paper was published by Mr Sharpe, in the 2d volume of the 2d series of the *Manchester Memoirs*. He shows in this paper; 1. That if the source of heat be the same or invariable, water heats equably or in the same times, from 120°, up to the highest temperature which it can reach without boiling. Suppose for example, that it is heated from 120° to 130°, or 10° in three minutes; then it will be heated from 270° to 280°, in exactly the same time. The reason of this equality probably is, that the difference between the temperature of the fire (the source of heat in these experiments) and the water was so great, that the 150° or 200° which had been added to the water, had no sensible effect in diminishing that difference. 2. That whatever be the temperature of steam from 212° upwards, if we take the same weight of it and condense it by water, the temperature of the water will always be raised the same number of degrees; or, in other words, the absolute quantity of heat is always the *same* in the *same weight of* steam, whatever the temperature of that steam is. M. Clement informed me a good many years ago, that he had established the same fact by a set of experiments of his own. And I have seen frequent allusions to these experiments of Clement in the French journals, though I am not aware that the experiments themselves have ever been published.

From these experiments of Mr Sharpe, confirmed as they

have been by those of Clement, it follows that the latent and sensible heats of steam added together, make a constant quantity; or, in other words, that the latent heat diminishes as the sensible heat increases, and *vice versa*. Thus, Mr Sharpe has proved that the truth which Mr Watt established under the boiling point, holds also above the boiling point, and that the proposition is universally true.

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Latent and sensible heats a constant quantity.

If we neglect all that portion of sensible heat which lies below 32°, we may say that at 212°, the sensible heat of steam is 180°, while its latent heat is 1000°, making together the sum of 1180°. And this sum being constant, we can easily determine what the latent heat of steam is at any temperature. We have only to subtract the sensible heat from the constant quantity 1180°. The following little table exhibits the latent heat of steam at different temperatures :—

Temp.	Latent heat.	Temp.	Latent heat.
32°	1180	180°	1032
36	1176	212	1000
56	1156	250	962
78	1134	293·4	918·6
100	1112	343·4	868·6
125	1087	419	793
150	1062		

It is obvious from this table, that while the specific gravity or elasticity of steam increases in a geometrical progression with a ratio of 2, the latent heats diminish in a geometrical progression whose ratio does not very much surpass unity, being probably 1·0306. We might therefore calculate the temperature at which the elasticity of steam increases in a geometrical progression whose ratio is 2, or carry the table given in page 186 to any length. The following will serve as a specimen of the way in which this might be done :—

Temperature.	Elasticity in inches of mercury.
293°·4	120
343·4	240
370·2	480
395·2	960
419·5	1920

It is obvious, however, that the elasticity by no means increases at so great a rate as we have supposed from the table

Chap. III. in page 186. I think there can be no doubt that somewhere about 350°, the increase of elasticity begins to follow a much smaller rate. For the elasticity at 419°, as we learn from the experiment of Perkins, instead of 1920, is only equal to 1050 inches of mercury.

If we could apply such a pressure to water that we could heat it till its sensible heat rose to 1212°, it is obvious that it would be converted into steam having the specific gravity, and consequently the volume of the original water. The latent heat of such steam would be 0°; but its elasticity would be prodigious. The instant that the pressure upon it was removed, it would expand, and its latent heat would increase at the expense of its sensible heat. It is obvious from this that the existence of latent heat in steam is owing to its expansion and the consequence of its expansion, and that the moment we reduce it to the bulk of the water from which it was generated, all the latent heat becomes sensible. We may conceive that water has a strong affinity for heat, that an atmosphere of it accumulates round every integrant particle of the water, and by its repellent property forces the aqueous particles nearly twelve times farther from each other than when they constituted water.

Latent heat of other liquids.

14. Few experiments have hitherto been made to determine the latent heats of other liquids. A few have been determined by M. Despretz and by Dr Ure, by the method employed by Mr Watt. Count Rumford's modification of the process constitutes a great improvement. He sunk the water in the refrigeratory 4° lower than the temperature of the room, and continued the distillation of the liquids under examination till the temperature of the water had risen 4° higher than that of the room. During the first part of the process, the water of the refrigeratory was imbibing heat from the room; during the last part of the process it was giving out heat to the room, and these two opposite currents must have just balanced each other. He then determined the weight of the liquid which had distilled over, and thus was able with ease to determine its latent heat. The following little table exhibits the latent heats of such vapours as have been examined:—

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	Latent heats.	Ditto referred to water.
m	1000°	1000°
hol vapour (sp. gr. 0·793)* .	597	375·86
huric ether (sp. gr. 0·715)* .	314	163·44
of turpentine*	299·16	138·24
nonia (sp. gr. 978)† . .	837·28	862
ic acid (sp. gr. 1·494)† . .	531·99	335
htha†	177·87	73·77

f we calculate the latent heats of these different vapours n their expansions we obtain the following results:—

	Latent heats.	Ditto referred to water.
Alcohol	578°	360°
Ether	236·7	123·1
Oil of turpentine . . .	246·7	114

As these are considerably below the experimental results, emains to be seen whether there be an error in the expanns, or whether the conclusion that the latent heat should proportional to the expansion be erroneous.

SECTION II.—OF GASES.

The word *gas* appears to have been introduced into che- stry by Van Helmont. He seems to have applied the term every thing which is driven off from bodies in the state of our by heat.‡ It was introduced into modern chemistry Macquer, who applied it to all those substances which Black, iestley, and Cavendish, and the British chemists in general, l called *airs*. Macquer was of opinion that atmospherical is a simple or elementary body, while those bodies to which term *gas* was applied by him, were in his opinion com-

Meaning of the term *gas*.

* Despretz; Ann. de Chim. et de Phys., xxiv. 323. It is probable that numbers given are rather under the truth.

† Ure; Phil. Trans. 1818, p. 388.

‡ He divides gases into five classes. "Nescivit, inquam, Schola Galenica differentiam inter Gas ventosum (quod mere aer est, id est ventus sydcrum blas commotus), Gas pingue; Gas siccum, quod sublimatum, Gas fuliginosum, sive endemicum, et Gas sylvestre, sive incoercie, quod in corpus cogi non potest visibile." Van Helmont de Flatibus, Opera Van Helmont, p. 399. Francofurti, 1682.

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pounds either of air with other bodies, or of certain bodies differing from air with each other.*

The term is at present applied to all substances which possess the mechanical properties of atmospherical air; that is to say, which are invisible and elastic, and capable of indefinite expansion and rarefaction without losing their aerial form. About 24 such bodies are at present known. The following table exhibits the names and specific gravities of these bodies, which I have determined by very careful and accurate experiments:—

Specific gravity of gases.

Gases.	Sp. gravity.	Weight of 100 cubic inches at 32° in grains.
Air	1·	32·79
Hydrogen	0·069$\dot{4}$	2·2756
Carburetted hydrogen	0·555$\dot{5}$	18·2148
Ammonia	0·5902$\dot{7}$	19·3011
Carbonic oxide	0·972$\dot{2}$	31·8790
Azotic	0·9722	31·8790
Olefiant	0·9722	31·8790
Deutoxide of azote	1·041$\dot{6}$	34·1543
Oxygen	1·111$\dot{1}$	36·4330
Sulphuretted hydrogen	1·1805	38·6196
Muriatic acid	1·2847$\dot{2}$	42·1260
Carbonic acid	1·527$\dot{7}$	49·9780
Protoxide of azote	1·527$\dot{7}$	49·9780
Cyanogen	1·805$\dot{5}$	59·2023
Phosphuretted hydrogen	1·845†	60·4976
Sulphurous acid	2·222$\dot{2}$	72·8660
Fluoboric acid	2·361$\dot{1}$	77·4205
Protoxide of chlorine	2·444$\dot{4}$	80·1519
Chlorine	2·5	81·9750
Chlorocarbonic acid	3·472$\dot{2}$	113·8530
Fluosilicic acid	3·611$\dot{1}$	118·4080
Hydriodic acid	4·3402$\dot{7}$	142·4160

* See his Dictionaire de Chimie; 2d edition, article *Gas*. Macquer always spells the word *gaz*, I know not for what reason, and in this he was followed for a time by the French writers in general. Van Helmont always spells the word *gas*, and this mode of spelling is now generally employed.

† By the determination of H. Rose.

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These gases are all invisible, except chlorine and protoxide of chlorine; both of which have a strong green or yellowish green colour.

Theory of gases.

Gases differ from vapours in this remarkable particular, that they do not lose their elastic state by the application of ordinary cold or ordinary pressure. Many philosophers have adopted the opinion that they owe this elastic state to the latent heat which they contain, as is the case with vapours. M. Amontons, an ingenious member of the French Academy of Sciences, about the beginning of the 18th century, was the first who proposed this idea with regard to the atmosphere. He supposed that it might be deprived of the whole of its elasticity, and condensed, and even frozen into a solid body, were it in our power to apply to it a sufficient cold—that it is a substance which differs from others in being incomparably more volatile, and which is therefore converted into vapour, and preserved in that form by a weaker heat than any that ever occurs, or can take place in our globe—and which, therefore, cannot appear under any other form than the one it now wears, so long as the constitution of the world remains the same as at present.

Though this opinion may appear at first sight an extravagant flight of the imagination, it is notwithstanding supported very strongly by analogy. We know that water is easily converted by heat into a vapour, which, so long as it is kept sufficiently hot, possesses all the mechanical or sensible qualities of air. Alcohol yields a vapour which retains its elasticity at a still lower temperature than steam. The vapour of sulphuric ether is as elastic as common air at the temperature of 96°. Muriatic ether has the form of a vapour, and is as elastic as common air at the temperature of 52°, so that we cannot preserve it in a liquid form in summer. Ammonia retains its elastic form, and possesses all the properties of a gas till it be cooled down to 46° below zero. In this climate, therefore, it always retains its elastic form, and is considered as a gas. But in Melville island, where Captain Parry wintered, it would assume the liquid form during a part of the

Chap. III. year, as the thermometer in February stood as low as 55° below zero.

Gases have been condensed into liquids.

This opinion of the similar constitution of vapours and gases, has been still farther confirmed by Mr Faraday, who, by the united action of pressure and cold, has succeeded in condensing eight different gases into liquids.* His method was, to shut up the materials for generating the gas in a strong glass tube hermetically sealed. Heat was applied (if necessary) to generate the gas at one end of the tube, while the other extremity was kept cool. The condensation occasioned by the accumulation of the gas in the tube, together with the cold, at last reduced the gas to the liquid state, and it was collected in the cold part of the tube. Thus mercury and concentrated sulphuric acid were sealed up in a bent tube, and being brought to one end, heat was carefully applied, while the other end was preserved cool by wet bibulous paper. Sulphurous acid gas was produced where the heat acted, and was condensed by the sulphuric acid present. But when that acid had become saturated, the sulphurous acid passed to the cold end of the tube, and was condensed into a liquid. The other liquid gases were generated, and condensed by similar contrivances.

The following table exhibits the names of the gases which Mr Faraday succeeded in condensing into liquids, with the pressure necessary to produce the liquefaction. This pressure is denoted by the number of atmospheres necessary to produce it, each atmosphere being equivalent to a column of mercury, 30 inches in height:—

	Atmospheres' pressure.	Sp. gravity.
Sulphurous acid	2 at 45°	1·42
Cyanogen	3·7 at 45	0·9
Chlorine	4 at 60	——
Ammonia	6½ at 50	0·76
Sulphuretted hydrogen	17 at 50	0·9
Carbonic acid	36 at 32	——

* Phil. Trans. 1823, p. 189.

	Atmospheres' pressure.	Sp. gravity.
Muriatic acid . . .	40 at 50° .	——
Protoxide of azote . .	50 at 45 .	——
Protoxide of chlorine .	Not determined .	——
Carburetted hydrogen .	1200*	

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Sir Humphry Davy has shown that the elasticity of the vapours, from these liquid gases, increases at a great rate with their temperature. Thus sulphuretted hydrogen, condensed in a tube at 3°, had a vapour floating over it, which exerted a pressure of 14 atmospheres; when heated to 47°, it exerted a pressure of 17 atmospheres. Liquid muriatic acid at 3°, exerted a force equal to 20 atmospheres; when heated 22° its force was equal to 25 atmospheres, and by a further addition of 25° its elastic force became equal to 40 atmospheres. Carbonic acid at 12° exerted a force of 20 atmospheres, and at 32° its elasticity was equal to 36 atmospheres. Liquid ammonia at 32° exerts a force of 5 atmospheres, and at 50° a force of 6½ atmospheres. Liquid protoxide of azote at 32° exerts a force of 44 atmospheres, and at 45° a force of 51·3 atmospheres nearly.†

Mr Faraday's attempts to condense hydrogen, oxygen, fluoboric, fluosilicic, and phosphuretted hydrogen gases, were unsuccessful. Indeed oxygen, azotic gas, and hydrogen, have been subjected to a pressure of 800 atmospheres, without any tendency to assume the liquid form. All attempts to condense them, therefore, by mere pressure, are likely to end in disappointment.

Absorption of gases by water.

But there are other phenomena connected with the gases, which may throw some light on their constitution. It is well known that water has the property of absorbing them, and that it absorbs a definite volume which characterizes each particular gas. Dr Henry ascertained, that the volume of each gas absorbed by water, is the same whatever be the pressure to which the gas is previously subjected. If we double the weight of carbonic acid, by subjecting it to the

* Perkins; Phil. Trans. 1826, p. 544.

† Phil. Trans. 1823, p. 200.

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pressure of two atmospheres, water will still absorb its ow volume of it. The following table exhibits the volumes ‹ each gas absorbed by 100 volumes of water, supposing th temperature and pressure the same in all cases:—

	Absorption in volumes.	Authority.
Cyanogen	450 .	Gay-Lussac
Sulphuretted hydrogen .	366·6 .	Thomson
Chlorine	200 .	Berthollet
Carbonic acid . . .	106 .	Cavendish
Protoxide of azote . .	76 .	Saussure
Olefiant gas . . .	15·3 .	Saussure
Phosphuretted hydrogen .	5 .	Thomson
Deutoxide of azote . .	3·7 .	Dalton
Oxygen	3·7 .	Henry
Carburetted hydrogen .	3·7 .	Dalton
Azote	2·5 .	Dalton
Carbonic oxide . . .	2·01 .	Henry
Hydrogen	2 .	Dalton

Depends upon the tendency to assume the liquid state.

Mr Thomas Graham has suggested that these gases, befo they can be absorbed by or combined with water, in all pr bability assume the liquid form. If this conjecture be admitte it is clear that the quantity of each absorbed must bear so relation to its elasticity. It may not be in the inverse rati exactly, because the amount of the affinity between the gas and water may and probably does differ considerably. Th is doubtless the reason why sulphuretted hydrogen is mo absorbable than chlorine, although chlorine is condensable b a pressure of 4 atmospheres, while sulphuretted hydrogen r quires 17 atmospheres to condense it. But we may conclu in general, that those gases which are but little absorbed b water, are much more difficult to condense into the liqu state by pressure than those which are absorbed in gre quantity, by that liquid. Accordingly we find that all t gases liquefied by Mr Faraday, are absorbed in considerab quantity by water. He succeeded with sulphurous acid, m riatic acid, and ammonia, which are absorbed by water in gre quantity, and likewise with the first five gases in the precedi table; but he failed with all the rest. It is not unlikely th

olefiant gas might also be condensed into a liquid, though the pressure requisite would be much greater than what was required to condense any of those that precede it in the table. But the very small absorbability of all the rest, renders it unlikely that any of them could be condensed into a liquid by simple pressure. We see indeed from Mr Perkins' experiment that carburetted hydrogen is condensed by a pressure of 1200 atmospheres. The same pressure would probably condense all the gases above it in the table. Sect. III.

Upon the whole, the notion that gases and vapours have the same constitution, and differ only in the obstinacy with which the former retain their latent heat, is supported by such strong analogies, and so many gases have actually been reduced to liquids by the united action of cold and pressure, that it has been generally adopted by modern chemists.

SECTION III.—OF SPONTANEOUS EVAPORATION.

Water evaporates at all temperatures.

Every body knows that water evaporates at all temperatures, however low. After a heavy fall of rain the roads become deep, and the country becomes studded with little ponds of water. But after a few days or weeks of fair weather, the roads get dry and dusty, and the little ponds of water disappear. And this takes place not only in summer but even in winter, when the weather happens to continue dry for some time. The Mediterranean sea receives many very large rivers. The Nile, the Po, the Rhone, the Ebro, the Danube, the Nieper, the Don, and many other rivers of smaller extent, empty themselves either directly into the Mediterranean or into the seas connected with it, and constituting as it were a part of this great inland ocean; yet, notwithstanding this great and regular influx of water, this sea not only does not increase in size, but a constant current sets in from the Atlantic through the Straits of Gibraltar—an evident proof that the natural evaporation from the surface of the Mediterranean, is more than sufficient to dissipate all the water thrown into it from a vast tract of Europe and Africa.

Water is not the only liquid that evaporates at temperatures

below the boiling point. Alcohol, ethers, bisulphuret of carbon, volatile oils, nitric acid, and some other liquids (even mercury), are in similar circumstances. Sulphuric acid, indeed, in the state in which it is prepared by our manufacturers, not only does not evaporate, but even absorbs moisture from the atmosphere and becomes much heavier. But when we expose sulphuric acid totally destitute of water (in which state it is a white solid) to the atmosphere, it evaporates at the common temperature with great rapidity, and speedily fills the apartment in which it is exposed with a white smoke, so thick and so offensive, that I have been frequently obliged to quit the apartment till the offensive vapour was dissipated by throwing open the windows. The fixed oils do not evaporate sensibly till their temperature is raised to a considerable height. Indeed, the term *evaporation*, as applied to them, is not quite correct, for they cannot be converted into vapour or distilled over without acquiring new properties.

Evaporation confined to the surface.

Let us consider the evaporation of water at low temperatures with attention.

1. This evaporation is entirely confined to the surface, and is therefore proportional to the surface. Hence, if the quantity of water to evaporate be constant, it will disappear the sooner the shallower it is, because its surface will be the greater. If we cover the surface of the water with oil, the evaporation is stopped altogether.

On the western coast of France, and in some places on the south coast of England, sea salt is obtained by allowing sea water to evaporate spontaneously, till the salt separates in crystals. The sea water is allowed to flow into a number of shallow, oblong divisions dug on the sea-shore, and lined internally with clay. The sea water in these pits presents a great surface, and in the hot weather of summer, it gradually evaporates away (provided the weather be dry), leaving the salt in crystals. Salt obtained in this way, is called *bay salt*. The crystals are large and hard, and not very white. Th[e] kind of salt is preferred to every other by the curers of fis[h]. At one time, the superiority of Dutch cured herrings was [as]cribed to the bay salt with which they were salted.

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2. Water does not evaporate so rapidly when the air is still, as when it is agitated by a brisk wind; and the more rapid the current of air the more rapidly, *ceteris paribus*, does the water disappear: hence, in manufactories where the evaporation of water constitutes a part of the process, the vessels containing the water to be evaporated, ought to be placed so that a brisk current of air shall pass over their surface. In Glasgow and the neighbourhood, soda and alum leys are brought to the requisite degree of concentration, by passing over their surface a brisk current of air which has previously passed through a fire; so that the flame of the fire plays over the surface of the evaporating liquor. And this process has been found more convenient and economical, than the common method of evaporating liquids, by applying heat to the bottom of the vessels containing them. Increased by wind,

When air is perfectly still the vapour as it forms accumulates over the surface of the water, and in proportion as this accumulation increases, does the process of evaporation diminish. So that at last it is reduced to nothing.

3. The rate of evaporation increases with the temperature. Evaporation goes on at 32°, or even at lower temperatures. For ice and snow are constantly emitting vapour, and diminishing in weight when surrounded by a dry atmosphere. If two or three ounces of ice be confined in the exhausted receiver of an air-pump over concentrated sulphuric acid, it evaporates all away in little more than 24 hours; and yet during the whole time the temperature of the ice is considerably lower than 32°. In the open air, and consequently under the pressure of the atmosphere, the evaporation of the ice, though quite sensible, is a great deal slower. and by heat.

Mr Dalton made a set of experiments to determine the rate at which water evaporates from a vessel of a given diameter at different temperatures. By observing the rate at which the evaporation increased or diminished according to the temperature, he was enabled to construct a table exhibiting the quantity evaporated in a given time at every temperature, from zero up to 212°—supposing the atmosphere in which the evaporation is going on to be quite free from vapour. He

Chap. III. found that the rate of evaporation was always proportional to the elasticity of the vapour generated, measured by the length of a column of mercury which it is capable of supporting. By referring to the table of the elasticities of vapour at different temperatures, given in page 186, it will be seen that the quantity evaporated from a given surface at the following temperatures is nearly as follows :—

Temperature.	Rate of evaporation.
212°	512
180	256
150	128
125	64
100	32
79·5	16
58	8
38	4
18·5	2

The following table, showing the force of vapour at all temperatures, from 32° to 80°, was constructed by interpolation from a set of experiments made with great care, by Mr Crichton, of Glasgow, to determine the force of vapour at 32°, 40°, 50°, and 60°. The three last columns of the table show the quantity of vapour in grains which would be driven off in a minute from a circular vessel of water six inches in diameter, as determined by Mr Dalton, according as it is placed in circumstances less or more favourable for evaporation.*

Table of the force of vapour, and rate of evaporation.

Temperature.	Force of vapour in inches of mercury.	Quantity evaporated per minute from a circular vessel 6 inches diameter in grains.		
		1	2	3
32°	0·2000	0·80	1·03	1·26
33	0·2066	0·83	1·07	1·30
34	0·2134	0·86	1·11	1·35
35	0·2204	0·89	1·14	1·39
36	0·2277	0·92	1·18	1·45
37	0·2352	0·95	1·22	1·49
38	0·2429	0·98	1·26	1·54
39	0·2509	1·02	1·31	1·60
40	0·2600	1·05	1·35	1·65
41	0·2686	1·09	1·40	1·71

* Manchester Memoirs, v. 585, 1st series.

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Tempera-ture.	Force of vapour in inches of mercury.	Quantity evaporated per minute from a circular vessel 6 inches diameter in grains.		
		1	2	3
42°	0·2775	1·13	1·45	1·78
[illegible]	0·2866	1·18	1·51	1·85
44	0·2961	1·22	1·57	1·92
45	0·3059	1·26	1·62	1·99
46	0·3160	1·31	1·68	2·06
47	0·3264	1·36	1·75	2·13
48	0·3372	1·40	1·80	2·20
49	0·3483	1·45	1·86	2·28
50	0·3600	1·50	1·92	2·36
51	0·3735	1·55	1·99	2·44
52	0·3875	1·60	2·06	2·51
53	0·4020	1·66	2·13	2·61
54	0·4171	1·71	2·20	2·69
55	0·4327	1·77	2·28	2·78
56	0·4489	1·86	2·35	2·88
57	0·4657	1·90	2·43	2·98
58	0·4832	1·96	2·52	3·08
59	0·5012	2·06	2·61	3·19
60	0·5200	2·10	2·70	3·30
61	0·5377	2·17	2·79	3·41
62	0·5560	2·24	2·88	3·52
63	0·5749	2·31	2·97	3·63
64	0·5944	2·39	3·07	3·76
65	0·6146	2·46	3·16	3 87
66	0·6355	2·54	3·27	3·99
67	0·6571	2·62	3·37	4·12
[illegible]	0·6794	2·70	3·47	4·24
[illegible]	0·7025	2·79	3·59	4·38
70	0·7260	2·88	3·70	4·53
71	0·7507	2·98	3·83	4·68
72	0·7762	3·08	3·96	4·84
73	0·8026	3·18	4·09	5·00
74	0·8299	3·29	4·23	5·17
75	0·8581	3·40	4·37	5·34
[illegible]	0·8873	3·52	4·52	5·53
77	0·9175	3·65	4·68	5·72
78	0·9487	3·76	4·83	5·91
79	0·9809	3·88	4·99	6·10
80	1·0120	4·00	5·14	6·29

Were we to suppose the atmosphere perfectly dry in both cases, the rate of evaporation ought to be four times greater in summer than in winter from the surface of lakes and the earth in this country. In reality the difference is much greater,

Chap. III. because the atmosphere is usually loaded with moisture in winter, and comparatively dry in summer. And to know the true rate of evaporation, we must always subtract from the quantity that would be evaporated what exists already in the atmosphere.

Evaporation most rapid in a vacuum.

4. Water evaporates much more rapidly in vacuo, provided the vapours be withdrawn in proportion as they are formed, than it does under the pressure of the atmosphere, supposing the temperature and all other things the same. This is beautifully exemplified in Professor Leslie's mode of producing ice, by placing a vessel containing water a little above a flat dish filled with sulphuric acid in the exhausted receiver of an air-pump. The sulphuric acid absorbs the vapours as fast as they are produced; the rate of evaporation is kept up, and so great a degree of cold produced that the water soon freezes, even in summer. The cold produced is here the criterion of the rapidity of evaporation. And in favourable circumstances Mr Leslie not only froze mercury by this process; but, if I understand him rightly, he sunk the thermometer to —120°.

5. Such are the constant phenomena of spontaneous evaporation. Let us now attend to the explanations of these phenomena which have been successively proposed by men of science.

It was supposed at first that the vapour which rises from water below the boiling point is quite different from the vapour of water produced at or above the boiling point. In particular it was supposed that this low temperature vapour is devoid of elasticity. This led to an explanation of spontaneous evaporation first suggested by Dr Halley, and afterwards explained in detail by Leroi of Montpellier, Dr Franklin, Dr Hamilton of Dublin, and several other men of science. According to Dr Halley water is *dissolved* by air in the same way as salt or sugar is *dissolved* by water. This hypothesis accounts for many of the phenomena of spontaneous evaporation in a simple and satisfactory manner. It gives us a reason why evaporation is so much promoted by wind—because the solvent is rapidly renewed. Heat increases the solvent power of air just as it does that of water.

Evaporation ascribed to solution.

But erroneously.

But when De Luc and Saussure demonstrated that vapour of

all temperatures is possessed of elasticity, and that it augments the volume of air when mixed with it exactly in proportion to its elasticity, the theory of Halley lost much of its plausibility. And after Saussure and Pictet had ascertained that evaporation goes on better in vacuo than under the pressure of the atmosphere, and that the vapour thus formed in vacuo possessed the same elasticity, and occupied the same volume, as when formed under the pressure of the atmosphere, the theory was no longer tenable. For how can spontaneous evaporation be considered as a solution of water in air when it goes on better without air? Or how could it be considered as a chemical union between air and water when it was known that the volume of vapour depended in all cases upon the temperature, and that the kind of air, or even the absence of all air, was a matter of indifference?

After these facts had become known towards the end of the last century, there was a general disposition among well informed chemists to abandon the theory of Halley. But it was the Essays and Experiments of Mr Dalton, published in 1802,* which completely established the true theory of spontaneous evaporation.

Method of determining the volume of vapour in the atmosphere.

Water and many other liquids have a tendency to assume the elastic form of vapours at all temperatures, however low. At low temperatures the elasticity of these vapours is small: hence, unless the particles of vapour be at a great distance from each other, they unite together again, and assume the form of a liquid. Thus the quantity of vapour which can exist in the atmosphere is regulated by the elasticity of that vapour; and this elasticity is measured by the height at which it is capable of supporting a column of mercury. We usually reckon the mean height of the barometer at the sea-shore in this country 30 inches.† At 32° vapour of water is capable of supporting a column of mercury 0·2 inch in height. It is clear, therefore, that at that temperature the utmost quantity

* Manchester Memoirs, 1st series, v. 535.

† I have already stated that the true height is a little lower than this, or 29·82 inches.

Chap. III. of the vapour of water which can exist in the atmosphere is about $\frac{1}{150}$th of its volume.

Mr Dalton has shown that if p = pressure of the atmosphere in inches of mercury, f = elasticity of vapour contained in the atmosphere, x = volumes of dry air in 100 volumes of the given atmospherical air. Then

$$\frac{p\,x}{p-f} = 100 \text{ ; consequently } x = \frac{100}{\frac{p}{p-f}}$$

Let air be saturated with moisture at 32°. In that case we have

$$p = 30 \qquad \frac{p}{p-f} = \frac{30}{29{\cdot}8} = 1{\cdot}00671$$
$$f = 0{\cdot}2$$

$$x = \frac{100}{1{\cdot}00671} = 99{\cdot}333.$$

So that the vapour in 100 volumes of such air has a volume amounting to 0·666, which is just $\frac{1}{150}$th of the whole.

At the temperature of 60°, $f = 0{\cdot}52$, we have therefore

$$\frac{p\,x}{p-f} = \frac{30\,x}{29{\cdot}48} = 100 \text{ ; and } x = \frac{100}{\frac{30}{29{\cdot}48}} = 98{\cdot}267$$

So that the whole vapour capable of existing in the atmosphere at the temperature of 60° is $\frac{1733}{100000}$.

The highest temperature that I have ever seen in Great Britain was 93°. At that temperature $f = 1{\cdot}5$

$$\frac{p\,x}{p-f} = \frac{30\,x}{28{\cdot}5} = 100 \text{ ; and } x = 95.$$

So that the vapour capable of existing in the atmosphere at that temperature is $\frac{5}{100}$, or $\frac{1}{20}$.

Thus we can easily determine the volume of vapour which is capable of existing in the atmosphere at any temperature.

A still easier method of obtaining the same volume, is to ascertain by the preceding table, the force of vapour at any given temperature. Let this number be made the numerator of a fraction whose denominator is the constant number 30. This fraction will denote the volume of vapour, capable of ex-

isting in the atmosphere, at the given temperature, supposing it reduced to the density of steam at 212°, or supposing its specific gravity 0·625. Thus at 60° the force of vapour is 0·52; its volume capable of existing in the atmosphere, at that temperature, is therefore $.\frac{52}{3000}$, or rather more than $\frac{1}{58}$th part.

Mr Dalton's method of determining the elasticity of vapour, at various temperatures, was this. He had a barometer tube, shut at one end, and open at the other, and quite dry. It was filled with newly-boiled mercury, inverted in a basin of mercury; the height of the mercury was accurately marked, and the tube was graduated into inches, and tenths, by means of a file. A little water was then poured into the tube in place of the mercury, so as to moisten the whole inside. Mercury was poured into the tube again, and it was inverted as before, into a basin of mercury. By degrees a thin film of water collected on the surface of the mercury. The mercury stood a little lower in the tube than before, and the difference of altitude indicated the elasticity of the vapour of water, at the temperature at which the observation was made; or the height of the column of mercury which it would be capable of supporting.

Mr Dalton then took a cylindrical glass tube, open at both ends, 2 inches in diameter, and 14 inches long. To each end of it a cork was fitted, perforated so as to admit the barometer tube to be passed through, and to be held fast. Into this cylinder he put water of different temperatures, and observed how far the top of the mercury was depressed by each. This depression gave the elasticity of the vapour at the given temperatures. Proceeding in this way, he constructed the table inserted in 186 of this volume; and by observing the rate at which the elasticity varied, he was enabled to extend his table down to a temperature as low as —40°.

Rate of evaporation depends on the quantity of vapour of air.

6. But the rate of evaporation, which has been just explained, holds good only when the atmosphere is perfectly dry, or when it contains no vapour whatever. If vapour be already present in the atmosphere, as is always the case in this country, then the quantity capable of evaporating at a given temperature, will be the quantity in the table diminished by the vapour already existing in the atmosphere. If we suppose the atmo-

Chap. III. sphere to contain as much vapour as is capable of existing in it at the given temperature, then it is obvious that no evaporation whatever would take place. If the quantity of vapour contained in the atmosphere be less than it can support, we must find how much less, and this quantity subtracted from the rate of evaporation at the given temperature, when the atmosphere is dry, will give us the true quantity which will evaporate under existing circumstances. Suppose the temperature to be 60°. At that temperature, the force of vapour is 0·52 inch, so that at 60° the atmosphere is capable of containing about $\frac{1}{37}$th of its volume of vapour. But let us suppose that the elasticity of the vapour already in the atmosphere is 0·26. We must subtract 0·26 from 0·52; the remainder 0·26 gives the elasticity of the steam that really can enter the atmosphere. Now, 0·26 represents the elasticity of steam at 40°. So that though the temperature be 60, yet in consequence of the vapour already existing in the atmosphere, no more water will evaporate from a given surface than would evaporate at 40°, if the atmosphere at that temperature were perfectly dry.

Various instruments have been contrived, to enable us to ascertain the quantity of vapour existing in the atmosphere by inspection. These instruments are distinguished by the name of *hygrometers*. The three most celebrated of these instruments are the hygrometer of De Luc, of Saussure, and of Leslie.

Hygrometers.

De Luc's hygrometer consists of an extremely thin piece of whalebone attached to an index, which by the whalebone swelling when it imbibes moisture, and contracting as it dries, is turned round, and thus points out the degree of moisture or dryness of the air.

Saussure's hygrometer consists of a human hair boiled in a caustic ley, and moving an index precisely in the same way by its expansions and contractions.

Leslie's hygrometer is merely his differential thermometer with one of the balls covered with silk and bibulous paper and kept moist. This moisture evaporating produces cold and contracts the volume of air within the ball: hence the col-

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red liquid rises in the tube under this ball. The instrument therefore measures the rapidity of evaporation. But this rapidity is always (other things being equal) directly as the temperature, and inversely as the quantity of moisture already in the atmosphere. This instrument, however, cannot point out the true quantity of vapour already in the atmosphere, because the rate of evaporation is not merely affected by the quantity of vapour already in the atmosphere, but likewise by every alteration in the temperature of the air.

Method of finding the quantity of vapour in air.

To determine the elasticity of the vapour in the atmosphere, the simplest method is one long ago employed by Leroi, and afterwards by Mr Dalton. It is founded upon a well-known property of glass. If a piece of glass one degree colder than the atmosphere, be surrounded with air holding as much vapour as it can hold at the given temperature, moisture will be deposited upon it. The method is this. Suppose we wish to determine the quantity of vapour actually in the atmosphere. We must repair either to the fields or at least to an open window; for were we to determine within doors our results would seldom be accurate. Provide a very thin glass tumbler, a thermometer and a quantity of water ten or fifteen degrees colder than the atmosphere. In summer the water from our springs, which in the neighbourhood of Glasgow have a temperature of about 47°·75, will answer very well. But in winter the water must be cooled with ice, or we must sometimes use a mixture of snow and salt, or when ice or snow cannot be had, we may easily reduce the temperature of the water low enough for the purpose by dissolving in it a quantity of pounded crystals of glauber salt or of carbonate of soda. Pour the water into the tumbler. Moisture is immediately deposited upon the outside of the glass (if the water be cold enough). Pour back the cold water into the vessel which originally contained it, and wipe the outside of the tumbler dry. When the water has become a little warmer, pour it again into the tumbler, and observe whether moisture be deposited on the outside. If it be, the water must be allowed to get a little warmer, and the trial again repeated. Proceed in this way till the water is just not cold enough to cause the condensation of vapour on the

Chap. III. outside of the glass. Over against the temperature water thus determined, in the preceding table will be the elasticity of the vapour in the atmosphere at the our observation. If the temperature of the air be 6(water of the temperature 50° is just incapable of cond vapour on the outside of the glass; then 50° represe temperature which measures the elasticity of the vapour atmosphere. Over against 50° in the table we find 0·3 which represents the elasticity of the vapour in the atmo at the time of the observation.

Mr Daniell's hygrometer is an instrument contrived t the above observation still more conveniently. The cooled by means of ether, and the temperature of th when moisture is deposited on it is, indicated by a therm which constitutes a part of the apparatus.

Mr Thomas Jones has contrived a still simpler instr It is merely a mercurial thermometer, with a large bulb is turned up by bending the glass tube into the shape A little below the bulb is tied a piece of silk. When strument is to be used, a few drops of ether are let fal the silk cloth. Cold is produced. The moment is w when moisture condenses on the top of the bulb. The of the thermometer at that instant is noted. It gives t perature which we look for in the table, and over agai placed the elasticity of the vapour in the atmosphere time of the observation.

After trying both Daniell's hygrometer, and Mr Jone mometer, I am satisfied that the results obtained by th not so accurate as those obtained by the simple tumb cold water, provided the tumbler be made of sufficien glass.

Indeed, it has been shown by Dr Apjohn, that Daniell's nor Jones' instrument give the dew point direc he has given a formula by means of which the dew poi be deduced from an observation made by them. His is as follows: $f'' = f - md \times \frac{p}{30}$; in which f'' is the ten vapour at the dew point, f its tension at the tempera

the hygrometer, d the difference between the temperature of the hygrometer and the air, and p the height of the barometer. m is a co-efficient depending upon the specific heat of air and the caloric of elasticity of its included vapour. Its arithmetical value is 0·01149.* Sect. III.

7. The quantity of vapour in the atmosphere at Glasgow, and doubtless in every part of Great Britain, is greater in summer than in winter. Yet the rate of evaporation is greater in summer than in winter; because during winter the atmosphere is much nearer the point of saturation than in summer. The following table exhibits the mean elasticity of vapour in the atmosphere in Glasgow during the year 1823; and likewise the evaporation, determined by the elasticity of the vapour evaporated, for every fortnight. The observations were made daily, with scrupulous attention to accuracy, by Dr Hugh Colquhoun, of Glasgow, who was at that time my assistant. Rate of evaporation in Glasgow.

		Elast. of vapour in atmosphere, inches mer.	Evaporation, inches mer.
May	1 fortnight	0·2707	0·1482
	2 ——	0·3494	0·1429
June	1 ——	0·3052	0·2259
	2 ——	0·2822	0·3232
July	1 ——	0·3526	0·2475
	2 ——	0·3819	0·2037
August	1 ——	0·4000	0·1854
	2 ——	0·3546	0·2415
September	1 ——	0·3790	0·1945
	2 ——	0·3404	0·1253
October	1 ——	0·3233	0·0766
	2 ——	0·3026	0·0798
November	1 ——	0·2808	0·0747
	2 ——	0·3232	0·0450

* Transactions of the Royal Irish Academy, vol. xvii., or Phil. Mag. (3d series) vii. 266. Dr Apjohn's formula has been objected to by Dr Hudson, (Ibid. p. 256). Dr Hudson's formula would require correction in consequence of some inaccurate data which it contains. Dr Mason has given another formula. His paper is very ingenious and deserves to be consulted. See *Records of General Science*, iv. 96. Melloni's method is founded upon an inaccurate basis. See Ann. de Chim. et de Phys. xliii. 39. Brunner has determined the moisture in air by passing it through dry chloride of calcium. See Poggendorff's Annalen, xx. 274.

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			Elast. of vapour in atmosphere, inches mer.	Evapora inches
December	1	----	0·2449	0·0[illegible]
	2	- - -	0·2453	0·0[illegible]
1824. January	1	——	0·2481	0·06
	2	-- -	0·2578	0·05
February	1	——	0·2468	0·06
	2	—--	0·2170	0·07

The following table exhibits the average elasticity of vap in the atmosphere at Glasgow, and the rate of evaporati monthly during the time in which the observations were ma

	Elasticity of vapour in atmosphere.	Rate of evaporation
1823. May	0·3113	0·1454
June	0·2937	0·2746
July	0·3077	0·2249
August	0·3766	0·2143
September	0·3596	0·1666
October	0·3126	0·0782
November	0·3020	0·0598
December	0·2431	0·0253
1824. January	0·2531	0·0568
February	0·2314	0·0720

Few observations on the due point have been kept in otl parts of the world. Mr Daniell has given us a table for Lond for three years. Dr Heinecken has marked the maximum a minimum dew point at Funchal in Madeira (about north la tude 32°½) monthly for the year 1828, as follows:—*

At Funchal.

	Maximum.	Minimum.	Rain in inche
January	65°	50°	4·08
February	56	50	1·64
March	65	48	1·68
April	63	45	3·35
May	69	51	2·14
June	70	54	0·21
July	72	61	0·10
August	73	63	0
September	75	69	1·39
October	74	56	0
November	72·5	54	2·56
December	67	50	0·52
			17·67

* Brewster's Journal (new series), i. 24

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We see that the quantity of rain has no relation to the height of the dew point. The year was uncommonly dry. The rain in 1825 was 20·43 inches; in 1826, 43·35 inches; in 1827, 18·15 inches. The mean temperature of Madeira is, according to Dr Heineken, 66°·3.*

In India.

Some valuable observations on the quantity of moisture in the atmosphere in the Deckan, constituting that part of Indostan between north latitude 16° and 22°, have been made by Colonel Sykes.† The dew point at sun-rise is 66°·58, and at half past nine A.M. it is 67°·56; the temperature of the air being respectively 73°·66 and 77°·53. Colonel Sykes calculates that a cubic foot of air contains 7·473 and 7·634 grains of water. The highest dew point observed was 76°. It occurred on the 21st October, 1826, at 4 P.M. A cubic foot of air contained 9·945 grains of water. At 76° the tension of vapour is 0·88 inch mercury; and 100 cubic inches of air at that temperature, according to Dalton's table, contains 1·645 grains of moisture. This would make the quantity of moisture in a cubic foot of such air 17·28 grains or almost twice as much as Colonel Sykes makes it. The moistest month in the year is July. During that month the dew point is only 4°·85 below the temperature of the atmosphere.

The mean dew point for the year 1827, was 60°·74, and the mean temperature 78°·5. The lowest dew point at sunrise on the 4th December was 44°. In another year in January, the dew point was 29°, the temperature of the air being 62°. In February, 1828, the dew point was 27°, and the temperature of the air 57°·5. At Loghur, on the 12th of March, the dew point was 27°, and the temperature of the air 67°. At Downd, on Beema river, the dew point was 29°, and the temperature of the air 90°. But the atmosphere is much moister at Bombay than at Poona, which lies at a distance from the sea.

In Poona hail showers are common, and the hailstones have frequently the size of marbles. Dew first appears towards the close of the monsoon or the last morning of Sep-

* Brewster's Journal (new series), i. 41.

† Phil. Trans. 1835, p. 161.

Chap. III. tember, and in cloudless nights. In December, the dews a constant and copious. Fogs are rare in the open count Along the Ghauts they prevail for six months in the ye but in the open country they are only seen from October February inclusive.

Importance of spontaneous evaporation.

8. The property which water has of evaporating spontaneou at all temperatures, is one of the most important in the wh economy of nature; for upon it the growth of plants, a the existence of living creatures upon the earth, depen The vapours thus continually rising, not merely from the s face of the sea, lakes, and rivers, but also from the dry la are again condensed and fall in the state of rain and dew. T rain penetrates into the earth, and makes its way out again springs. These collecting together constitute rivers, whi making their way to the sea, afford the means of living a enjoyment to numerous tribes and languages which occu their banks. Let us suppose for a moment that this spont eous evaporation were to cease, and let us contemplate th consequences. No more rain or dew could fall, the spring would cease to flow, the rivers would be dried up; the whol water in the globe would be accumulated in the ocean; th earth would become dry and parched; vegetables being de prived of moisture, could no longer continue to grow; th cattle and beasts of every kind would lack their usual foo man himself would perish; the earth would become a dull, i animate, sterile mass, without any vegetables to embellish surface, or any living creature to wander through its fright deserts.

If the atmosphere contained no vapour whatever, the amo evaporation from the surface of water could easily be dete mined from the data already stated in this section, provid we were acquainted with the mean temperature of the pla But as the atmosphere is never free from vapour, we mu either determine the mean quantity present by trial, or dete mine the actual evaporation by experiment. Now, as far evaporation is concerned, the surface of the globe prese three principal varieties; namely, water, ground covered wi grass or other vegetables, and bare soil.

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Evaporation from water.

Dr Dobson made a set of experiments during the years 1772, 1773, 1774, and 1775, to determine the evaporation from the surface of water at Liverpool during these years. He took a cylindrical vessel of 12 inches' diameter, and having nearly filled it with water, exposed it beside a rain gauge of the same aperture, and by adding water, or removing it occasionally, he kept the surface at nearly the same height. By carefully registering the quantities added or taken away, and comparing them with the rain that fell, the amount of evaporation was ascertained. The mean annual evaporation from the surface of water at Liverpool amounted to 36·78 inches. The mean annual fall of rain at Liverpool, as ascertained by Dr Dobson, is (without reckoning the dew) 37·48 inches. We see at once from this that more rain falls at Liverpool than can be accounted for by the evaporation. Consequently there must be a supply of vapour from the sea, and probably from the warmer regions of the globe.*

From soil.

A set of experiments upon the evaporation from ground covered with vegetables and from bare soil was made by Mr Thomas Hoyle and Mr Dalton, at Manchester, during the years 1796, 1797, and 1798. They got a cylindrical vessel of tinned iron ten inches in diameter, and three feet deep. There were inserted into it two pipes turned downwards for the water to run off from it into bottles. One of these pipes was near the bottom of the vessel, the other was an inch from the top. This vessel was filled up for a few inches with gravel and sand, and all the rest of it with good fresh soil. It was then put into a hole in the ground, and the space around filled up with earth, except on one side for the convenience of putting bottles to the two pipes. Water was poured on to sadden the earth, and as much as would was suffered to run through without notice, by which the earth might be considered as saturated with water. For some weeks the soil was constantly above the level of the upper pipe, but latterly it was always a little below it; which made it impossible for any water to run through the upper pipe. For the first year the soil at top was bare, but during the last two years it was covered with grass

* Phil. Trans. 1777, p. 244.

the same as a green field. Things being thus circumstanced, a regular register was kept of the quantity of rain water that ran off from the surface of the earth by the upper pipe (when that took place), and also of the quantity which sunk down through the three feet of earth, and ran out through the lower pipe. A rain gauge of the same diameter was kept close to it to find the quantity of rain for any corresponding time. By this apparatus the quantity evaporated from the earth in the vessel during three years was ascertained. The annual evaporation was 25·158 inches. Now, if to the rain we add 5 inches for dew (not reckoned in Mr Dalton's observations), it follows that the mean annual evaporation from earth at Manchester amounts to 30 inches. It follows, likewise, from these observations of Dalton and Hoyle, that there is but little difference between the evaporation from *green soil* and bare soil; for the evaporation during the first year, when the soil in the vessel was bare, differed but little from that of the two following years when it was covered with grass.*

The mean annual evaporation at Glasgow may be estimated from the table which has been inserted in page 219 of this work. The mean elasticity of the vapour evaporated is 0·1283 inch; and it is probable that the elasticity for March and April, the two months not given, will not deviate much from the annual mean. Now, 0·1283 is the elasticity of the vapour of water at the temperature of 20°. From Mr Dalton's table inserted in page 210, it may be estimated that at the temperature of 20° a circular vessel of water, 6 inches in diameter, if exposed to a perfectly dry atmosphere, would lose per minute 0·44 grains of its weight by evaporation. This in a year would amount to 916$\frac{1}{3}$ cubic inches. Hence it follows by a very easy calculation that the annual evaporation at Glasgow amounts to 32·413 inches.

I am disposed to take 32 inches as the mean annual evaporation all over Great Britain. It is certainly less than this in a great part of Scotland, and a portion of England; but I am persuaded that it exceeds it considerably in the south-east

* Manchester Memoirs, 1st series, v. p. 360.

of England, and probably also in East-Lothian and Berwickshire. Let us now see how the rain and dew correspond with this evaporation. Sect. III.

Annual fall of rain.

9. The quantity of rain which falls annually in any place is determined by means of a rain gauge. This is usually a circular or square vessel having a surface of a determined number of square inches. It is funnel-shaped below, and terminates in a pipe which conducts the rain water to a bottle or some other vessel, where its quantity may be accurately measured. This rain gauge is set up in a field at a distance from every thing that may screen it or have a tendency to keep off the rain. When rain falls it enters into the vessel in proportion to the size of the surface, and being measured, and the quantity divided by the number of inches in the area of the rain gauge, the quotient gives the depth of rain which has fallen in the place where the gauge is fixed.

Rain gauges.

There have been many disputes about the accuracy of rain gauges. It has been affirmed that the quantity of rain which falls into them depends entirely upon the wind, and that, therefore, we cannot know by means of them how much rain falls in any place, unless they be so constructed that their surface shall always be perpendicular to the wind that blows. But it seems to me evident, that if a rain gauge be placed horizontally, and in a situation completely exposed on every side, and neither screened nor overlooked by any neighbouring building, exactly the same proportion of rain will fall into it as on the same area of the surrounding ground. Consequently it must (if the rain which falls into it all remain) give us a correct notion of the rain falling in the place where it is situated.

There are, however, some circumstances connected with rain gauges, that require examination and elucidation before we could trust entirely to the results which they give us. Professor Bache, of Philadelphia, stated at the annual meeting of the British Scientific Association, which met at Newcastle in 1838, that if two rain gauges, exactly alike, were placed at the two opposite angles of a square tower, that one on the side next the point from which the wind blows will indicate nearly twice as much rain as the other.

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Less rain falls in high than low places.

There is also a curious fact, first pointed out by Dr William Heberden, which ought to be taken into account. He placed a rain gauge on the square part of the roof of Westminster Abbey, at such a distance from the western towers as probably to be little affected by them. Another rain gauge was placed on the top of a house, but considerably lower than the first; and a third rain gauge on the ground in a garden belonging to the house. The observations were continued from July 1766, to July 1767, or a year. The rain water collected in each rain gauge was as follows:—

Top of Westminster Abbey .	12·099 inches.
Top of a house . . .	18·139
On the ground . . .	22·608*

This observation has been frequently repeated, and it has been found to hold constantly. More rain is always collected in the lower than in the upper gauge.

The observations of Dr Heberden were repeated by Messrs Philips and Gray, in the year 1832, at York. A rain gauge was placed on the top of York Minster at the height of 241 feet, 10½ inches above the river Ouse; another on the top of the Museum, at the height of 72 feet, 8 inches; and a third on the ground, 29 feet above the river, and the quantity of rain collected in each during the year 1832 was measured. It was as follows:—

York Minster .	15·91 inches.
Museum . .	20·461
Ground . .	24·401

They have given the following empyrical formula for calculating the quantity according to the height. Let $h =$ the height, then the diminution of rain will be $= m \sqrt{h}$; m being a coefficient which varies, being, in different circumstances, 2·29, 2·88, 1·97, 3·06, &c.† It is obvious from the variableness of m, that the circumstances connected with this diminution as the height increases, have not been all appreciated.

* Phil. Trans. 1769, p. 359.

† Third Report of the British Scientific Association, p. 401.

The annual quantity of rain is greatest at the equator, and gradually diminishes as we approach the pole. This will be evident from the following little table, showing the annual depth of rain in different latitudes:— Sect. III.

	Latitude N.	Rain in Inches.
Moulmain . . .	16°	197·12*
Marmato . . .	5 27′	64·09†
Granada . . .	12	126
Cape François . .	19 46	120
Calcutta . . .	22 23	81
Rome	41 54	39
Geneva . . .	47 20	30·4‡
England . . .	50° to 55°	31
Petersburg . . .	59 16	16
Uleaborg . . .	65 1	13½
George Town, Demerara	6 40	133·56§

On the contrary, the number of rainy days is smallest at the equator, and increases in proportion to the distance from it. From north latitude 12° to 43° the mean number of rainy days is 78. Number of rainy days.

From 43° to 46° the mean number is 103
—— 46 50 134
—— 50 60 161

It is obvious from this that the rain falls in very heavy showers in warm climates, and that it becomes more and more gentle as we advance towards the pole. Consequently in hot climates the air must be much less loaded with moisture, and the rate of evaporation much greater than in cold climates. For when rain falls very heavy a great portion of it runs off by the surface, and flows into the sea. But this happens to a much less extent when the fall of rain is more gentle.

But instead of attempting to compare the quantity of rain and evaporation over the whole surface of the globe, for which Rain in Great Britain.

* From 1st October, 1828, to 1st October, 1829: observed by Dr Anderson.

† It is 4678·5 feet above the level of the sea. Rain in 1833, 60·79 inches; in 1834, 67·40 inches. Boussingault, Ann. de Chim. et de Phys. lxi. 167.

‡ Mean of 32 years' observations. Saussure, Ann. de Chim. et de Phys. xliv. 32.

§ From 1st Jan. to 21st Sept. 1831.

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we have not sufficient data, let us confine ourselves to Great Britain. The annual evaporation for Great Britain is probably about 32 inches. The following table exhibits the mean annual fall of rain in different places in Great Britain:—

Manchester	36·140 inches
Liverpool	34·119
Chatsworth	27·664
Lancaster	39·714
Kendal	53·944
Dumfries	36·919
Glasgow	21·331
Edinburgh	24·5 nearly
Kinfauns Castle, near Perth.	
(1) On a hill, 600 feet above the sea	39·61
(2) In the garden, 20 feet do. .	25·19
(3) On Kinfauns castle, 129 feet do.	20·76
London	24·04*
Mean	30·33

But I have not inserted the mean rain at many places wher it is very considerable. At Plymouth, for instance, it is nc less than 44 inches. The annual fall in the Stockey-muir about 12 miles from Glasgow, is about 42·6 inches; at Greenock about 42·6 inches; at Largs about 43½ inches; and a Gordon Castle about 29·3 inches. I think, therefore, tha (including the dew) the mean fall of rain over all Great Britain cannot be estimated at less than 36 inches.

Excess above evaporation.

If this estimate be a near approximation to the truth, th evaporation is less than the rain by about 4 inches. Thi excess must be supplied from the neighbouring seas. The fou inches of rain not again elevated in the state of vapour mus be annually carried into the sea by means of the different rivers.

Now a quantity of water which would cover the whole sur face of Great Britain to the depth of 4 inches (supposing th surface level) would amount to 1,238,784,152,000,000 cubi inches, which is equal to 4,467,725,610,767 imperial gallon

* Luke Howard. Mean of 20 years' observations, from 1797 to 1816

or 17,729,069,884 tons, supposing the ton to contain 252 imperial gallons.* Sect. III.

Let us compare this with the quantity of water actually conveyed to the sea by some of the principal rivers in Great Britain.[1]

Quantity of water in the Clyde.

The breadth of the Clyde at the New Bridge, Glasgow, is 410 feet, and its mean depth $3\frac{1}{2}$. The velocity of the water at the surface is 1·23 inches, and the mean velocity of the whole water is 0·558,132 inch per second. From these data, it may be inferred that the quantity of water discharged per second is $76\frac{2}{3}$ cubic feet. This amounts to 2,417,760,000 cubic feet, or 473,017,448 imperial gallons, or 1,877,053 tons. The river Clyde drains about $\frac{1}{30}$th of Scotland, or about $\frac{1}{83}$d part of Great Britain: hence if the water discharged into the sea by the Clyde afforded a fair average of the whole island, the total amount of the water discharged annually by all the rivers in Great Britain would be only 155,795,399 tons, which does not amount to one-hundredth part of the excess of the rain above the evaporation.

But there are two circumstances which serve in some measure to account for the enormous difference between the two quantities. The water discharged by the Clyde was determined in summer, when the river is always much smaller than during winter. It is therefore considerably under the real quantity, though I have no data to determine how much. The district drained by the Clyde is not nearly so rainy as many other tracts both in England and Scotland: hence the water which it discharges should be less than the average for Great Britain. To this may be added that the size of the tract of country drained by the Clyde is somewhat arbitrarily estimated at $\frac{1}{30}$th of Scotland. The want of good maps makes it very difficult to determine how much it is in reality.

If our estimate of the difference between rain and evaporation be an approximation to the truth, it follows that $\frac{1}{9}$th of all the rain that falls is raised by evaporation from the sea.

* In making this calculation I have supposed the area of Great Britain to be 77,244 square miles.

Chap. III. This is one of the reasons why the most rain falls on th coast of Great Britain when the wind blows from the west and west, and most upon the east coast when it from the east. As the west wind blows for a much period of the year than the east wind, more rain falls west than the east coast of Great Britain. Glasgow, is an inland city, and at some distance from the mou is drier than Edinburgh, which is more contiguous Eastern Ocean, and much drier than Greenock or Largs, are contiguous to the Western Ocean.

10. While the moisture present in the atmosphere co in the state of vapour it is perfectly transparent and colo or at least it only communicates to the sky that live colour which we observe it to possess when the atmosp loaded with vapour. This colour seems owing to th perty which vapour has of absorbing the least refrangib of light, and transmitting only the most refrangible. is well known that the aspect of the sky becomes dark darker in proportion as we ascend in it; and when we such a height that all the aqueous vapour is left below sky appears to the eye perfectly black. This is the which it has from the top of Mont Blanc, as describ Saussure, and likewise from the summits of the Andes.

Blue colour of the sky.

But though the presence of vapours cannot be dete the atmosphere by their colour, yet we can easily judg the air is loaded with them; because we can see to a greater distance, and the outline of the distant mount much more distinct than when the air is dry. In this c it is a comparatively rare thing to see the sky perfectly parent. Much more frequently the vapour assumes a form, or becomes that opaque fleecy-looking matter wh denominate a *cloud*. Sometimes these clouds are very very thin, and very small in size. At other times the lower down and envelope the whole face of the sky. times two, or even three layers of clouds can be seen ferent heights above each other. Whenever this happe of the layers is moving with more velocity than the oth we observe one layer moving in one direction, and anot

Clouds.

a different one. Sometimes these clouds sink down to the very surface of the earth, and envelope it, in which case they are known by the name of *mists* or *fogs*. Sect. III.

There seems little reason to doubt that the cause to which Dr Hutton ascribed the formation of rain, is in reality the cause of the formation of clouds. Dr Hutton showed, by experiment, that when air of different temperatures, each containing as much vapour as is compatible with the temperature, are mixed together, a precipitation of moisture always takes place. A simple inspection of the table of the force of vapour given in page 210 will be sufficient to satisfy us that this must always be the case. How formed.

The force of vapour at . .	40° is	0·26 inch.
do. do. . .	60° is	0·52
The mean of which is . .		0·39
But the force of vapour at .	50° is	0·36

Now since 0·36 is less than 0·39, it is clear that whenever two such strata are mixed, a precipitation of moisture must always take place. Again,

The force of vapour at . .	60° is	0·52 inch.
do. do. . .	80° is	1·01
The mean of which is . .		0·76
But the force of vapour at 70° is only		0·726

showing clearly that when air at 60° and 80°, each saturated with moisture, are mixed, moisture must be precipitated.

Now, whenever moisture is precipitated from air, in consequence of the mixture of air of different temperatures, this precipitated moisture always assumes the appearance of a cloud or *mist*. We have a good example in the cloud formed when steam issues from the spout of a boiling tea-kettle. The steam has the temperature of 212°, and we shall suppose the air at 60°, and saturated with moisture.

Force of vapour at . .	60° is	0·52 inch.
do. do. . .	212° is	30
The mean of which is . .		15·26

But the force of vapour at 136°, the intermediate tempera-

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ture, is only 5·14 inches: hence a great proportion of the vapour must be precipitated. But this precipitated vapour, instead of assuming the form of drops of water, which one would have expected, is converted into a *cloud* or *mist*.

Nature of clouds.

Now *mist* or a *cloud* consists not of solid drops; but of a multitude of very thin vesicles of water, enclosing some aerial substance within them, similar to the vesicles usually blown from soap suds. That this is the structure of clouds and mists was affirmed by Derham and others, his contemporaries. Derham informs us, that he examined them by means of a microscope, and found them to be vesicles.* Indeed, the vesicular structure is obvious from the circumstance of clouds continuing elevated at a considerable height in the atmosphere, and that fogs may be often seen elevating themselves up the sides of mountains. If clouds or fogs consisted of round *drops*, their precipitation would be rapid. For a drop whose diameter amounted to $\frac{1}{1000}$th of an inch would acquire a velocity of nine or ten feet per second. Whereas we see clouds hover at a small elevation for hours, and they can in consequence be transported from the sea, lake, river, or marsh, from which they are raised, far into the country or to the tops of mountains, where the requisite supply of moisture cannot be had any other way.

Clouds composed of vesicles.

M. de Saussure, senior, while travelling in the Alps, happened to be enveloped in a mist which was almost stagnant. He was astonished at the size of the drops, as he thought them, and at seeing them floating slowly past him without falling to the ground. Some of them were larger than the largest peas. Catching them in his hand he found them to be bladders, inconceivably thin, and he found upon examination that they all had this structure. Indeed the optical phenomena exhibited by clouds and mists, show clearly that they are of a vesicular structure. If clouds were composed of drops they would exhibit a rainbow every time the sun shone upon them, supposing the spectator placed between the sun and the cloud. No such appearance, however, ever takes

* Derham's Physico-Theology, p. 48.

place. But every person must have observed the *halos* which are formed when a portion of fog is interposed between our eye and the sun or moon. The same halo may be perceived when the cloud formed by the mixture of steam and air is interposed between our eye and a candle or the sun. According to the measurements of De Saussure, the diameter of the smallest of the vesicles, of which clouds are usually composed, is about $\frac{1}{4222}$, and of the greatest about $\frac{1}{2620}$ of an English inch.

But though there is no doubt that clouds consist of a congeries of vesicles, we have no conception of the way in which these vesicles are formed. Nor is it easy to conceive why these vesicles are sometimes lighter than air, sometimes a little heavier, and sometimes exactly of the same specific gravity with the air in which they float. Indeed, if the aerial matter with which these vesicles are filled were saturated with moisture, while the air in which they float is dry, we would see a reason why they should be lighter than air. On such a supposition the clouds should rapidly disappear. Accordingly we generally find that when clouds rise in the atmosphere they speedily diminish in size, and at last vanish away; being gradually converted again into vapour. If the air within the vesicles were in the same state with respect to moisture as the air in which the cloud floats, the vesicles should be heavier than air, and constitute what we distinguish by the name of *fogs*.

The most probable cause of the difference of gravity between clouds and the air in which they float is a difference in their temperature from that of the surrounding medium. The formation of clouds seems to be connected with electricity, though in what way the vesicular form is induced by electricity, we have no conception. The vesicles seem to be all charged with the same kind of electricity. This causes them to repel each other, and of course prevents them from uniting into drops of rain.

Clouds then are formed whenever two strata of air of different temperatures, and each saturated with moisture, are mixed together. The sky in this country is usually much

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freer from clouds when the wind blows from the east than when it blows from the west: because the temperature of the east wind being low, there is less chance during its continuance of strata of different temperatures mixing together. The comparatively high temperature of the west wind renders the chance of an intermixture of air of different temperatures during its continuance much greater. Should a west wind blow in the upper regions of the atmosphere, while an east wind blows near the earth, the whole sky would become clouded, because the contiguous portions of the two strata mixing together would occasion a deposition of moisture.

Formation of rain.

11. The formation of rain is still involved in impenetrable obscurity. Rain never falls in this country unless the sky be cloudy, and unless that peculiar kind of dense black cloud appear well known by the name of *rain cloud.* Whenever the particles constituting clouds lose their vesicular form and unite together in drops, rain falls. This change is probably connected with some electrical phenomena which are not yet understood. Clouds are attracted by mountains, and more rain falls in mountainous districts than in any other. We can conceive the mountain in the opposite electrical state from the cloud. This would account for the attraction. When the cloud came close to the mountain its electricity would be abstracted, and the vesicles in consequence might collapse into drops.

No rain in the Valles.

12. In that part of Peru called Valles, which lies on the north and south side of Lima, in south latitude 12°, bounded on the east by the Andes, and on the west by the Pacific Ocean, it never rains at all; but during winter the earth is covered with so thick a fog as to intercept the rays of the sun. This fog appears almost every day during winter with a density that obscures objects at any distance. About 10 or 11 o'clock it begins to rise, but without being totally dispersed, though it is then no impediment to the sight, intercepting only the direct rays of the sun by day, and that of the stars by night. Sometimes it is so far dispersed that the disc of the sun becomes visible, but the heat from his rays is still precluded. In the winter season these vapours dissolve into

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a very small mist or dew, which they call *garua*, and thus everywhere moisten the earth. These garuas never fall in quantities sufficient to damage the roads or incommode the traveller; but they render the most arid and barren parts fertile. They convert the disagreeable dust in the streets of Lima into mud.

Now, in that country the wind always blows from the south; that is from a colder to a warmer region. Sometimes it veers a point or two to the east; but it always blows between the south and south-east. When the fogs come on the south wind is barely felt, and a scarcely perceptible air seems to come from the north, which forms the fog.*

The obvious reason why it never rains in that country is that the wind constantly blows from a colder to a hotter part of the world. We see also the cause of the fogs. They are occasioned by the mixture of the hot air from the north with the colder air from the south.

Rain is produced by irregular winds. If the winds were always to blow steadily in the same direction no rain whatever would fall.

When a country is quite flat, as is the case with Egypt, it seldom rains, although the winds are not quite steady. In Egypt it very seldom rains. During June, July, August, and part of September, the wind blows from the north. During the latter part of September it blows from the east. The winds are most variable about the winter solstice. From that to March they are mostly southerly.† (Rarely rains in Egypt.)

The heavy rains that fall in India always take place during the shifting of the monsoons, and while they last the winds are always variable. Even in this country steady dry weather is always accompanied by a steady direction of the wind, whereas in rainy weather the winds are unsteady and variable.

These facts are sufficient to show the connexion of rain with the variable nature of the winds. (Rain owing to variable winds.)

* See Ulloa's Voyage, vol. ii. † See Volney's Travels.

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SECT. IV.—OF THE ATMOSPHERE.

It may not be improper to introduce here a few circumstances respecting the atmosphere, though rather loosely connected with the subject of this chapter; the object of which is to state the facts connected with the conversion of liquids into vapours or elastic fluids.

What is called the *atmosphere*, is the invisible aerial body which surrounds the earth to a considerable height, and which revolves round the axis of our planet like the earth and the ocean.

Constitution.

It is a mixture, or rather a compound, of two gases, *oxygen* and *azote*, containing 20 volumes of the former, and 80 volumes of the latter. But it always contains, besides, a small admixture of carbonic acid gas, and more or less water in the state of vapour, which of course modify somewhat the quantity of oxygen and azote in 100 cubic inches of air.

The quantity of carbonic acid gas in air is very small, scarcely amounting to $\frac{1}{1000}$th of the volume of the air; and it never increases, though enormous quantities of it are constantly produced and thrown into the atmosphere.

Quantity of carbonic acid formed.

Every individual, at an average, by breathing, throws 272 cubic feet of carbonic acid gas into the air in 24 hours. If the population of Glasgow be 260,000, the quantity of carbonic acid gas thrown by them into the atmosphere would in 24 hours amount to 78,798,510 cubic feet; if we admit the other animals, horses, cattle, dogs, cats, birds, &c., to amount to $\frac{1}{10}$th of 260,000, they would produce about 8,000,000 cubic feet more, so that the whole carbonic acid produced in Glasgow by breathing, in 24 hours, is 86,798,510 cubic feet.

If we suppose the consumption of coals in Glasgow and the neighbourhood to amount daily to 2000 tons, this will produce 1,078,510 cubic feet of carbonic acid; so that the whole carbonic acid gas thrown into the atmosphere in Glasgow daily must amount to 87,877,020 cubic feet.

Every volume of carbonic acid gas produced, renders 5 volumes of air unfit for respiration: hence, in 24 hours

439,385,100 cubic feet of air are rendered unfit for respiration, or in fact perfectly poisonous. Sect. IV.

Now, a base of 4 square miles, with a height of 100 feet, contains 44,605,000,000 cubic feet, of which 8,921,000,000 cubic feet are oxygen gas; consequently, in little more than 17 days the whole oxygen in that space would be converted into carbonic acid gas, and every creature in Glasgow, and its environs, would be destroyed. Yet if we examine the atmosphere in Glasgow or its neighbourhood, we find it always to contain the usual quantity of oxygen gas, and the proportion of carbonic acid never exceeds $\frac{1}{1000}$th of the volume—a proof that both the carbonic acid and the azote of the air are dissipated with extreme rapidity. Were this not so, the atmosphere near the surface of the earth would long ago have been unfit for supporting animal life.

Height of the atmosphere.

The quantity of atmospherical air is such as to equal the weight of a hollow sphere of quicksilver, thirty inches thick, and covering the whole surface of the globe. Now, as air is 3425·9 times lighter than mercury, it follows that if its density in the higher regions were as great as at the surface of the earth, the height of the atmosphere above the surface of the earth would be only 1·64 mile. But it is well known that the density of the air diminishes as we ascend; and if we reckon the height in an arithmetical progression, the diminution of density will go on in a geometrical progression.

It is said that air-pumps have been constructed so excellent that they were capable of rarifying air 1000 times. Now, at the height of 42 miles this would be the rarity of the atmosphere; so that if a barometer were elevated 42 miles above the surface of the earth, the height of the mercury in it would be rather less than $\frac{1}{1000}$th of an inch.

Kepler pointed out a very ingenious way of determining at what height the atmosphere ceases sensibly to refract light. Twilight ceases when the depression of the sun below the horizon amounts to 18°. Now, it is easy to deduce from this, that the atmosphere, when its height exceeds 49 miles, ceases sensibly to refract light. This of course is the height of the atmosphere as far as utility is concerned. But it is extremely

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probable that it extends farther than this. Nor is there any difficulty in assigning its absolute limit. The higher parts of the atmosphere are carried round the earth in 23 hours and 56 minutes, by the rotation of the earth about its axis. Now, these parts would be projected from the earth, and would leave it altogether if their centrifugal force were greater than their gravitation to the centre. But the centrifugal force varies directly as the distance, while gravitation varies as the square of the distance. It can easily be shown, that these two forces become equal at the distance of 6·6 radii of the earth, or about 26,000 miles: hence we are sure that no part of the atmosphere can be farther from the surface of the earth than 26,000 miles.

Mean height of the barometer.

The mean height of the barometer which measures the quantity of atmospheric air, is, at the surface of the sea and at the temperature of 32°, 29·9546 inches. So that at the temperature of 60°, the mean height of the barometer at the level of the sea is 29·9869 inches, or only 0·013 inch less than 30 inches.*

Variation of the barometer.

When we continue to observe a barometer in the same place, every body knows that it does not remain stationary, but rises and falls. This alteration in height is connected with the winds. Accordingly, in the torrid zone, where the winds are pretty regular, the rise and fall are very trifling, amounting only to a few tenths of an inch. At Madras, for example, the mean range of the barometer is only 0·6 inch. The greatest annual range ever observed in that place, was 0·964 inch, and the smallest 0·462 inch.

As we advance northwards or southwards from the equator, the range increases. In Glasgow it amounts to 2·95 inches, and at St Petersburg it exceeds 3 inches.

It has been observed that there are two periods of every day when the barometer is highest, and two corresponding periods in which it is lowest. These maxima and minima

* In consequence of this near approach, we reckon usually in this country the mean height of the barometer at the level of the sea, 30 inches. In France they consider it at 0·76 metre, which is equivalent to 29·92196 inches; so that the French determination is not so near the truth as ours.

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s are observed in all parts of the earth nearly at the same of the day; showing that they originate in *tides* of the sphere, occasioned by the action of the sun.

Has two daily maxima and minima.

ie barometer is highest of all between nine and ten in the ing. It has another maximum, but not quite so high, be- n ten and eleven at night. Its lowest points are between and five in the evening, and four and five in the morning.

From between ten and eleven in the morning, the baro- continues to fall till between four and five in the even- when it reaches its point of greatest depression.

From between four and five in the evening, it continues se till between ten and eleven in the evening, when it ies its second maximum point.

From between ten and eleven in the evening, it continues ll till between four and five in the morning, when it ies its second minimum point.

Finally, it rises from between four and five in the morn- till between nine and ten in the forenoon, when it is highest l.

hese oscillations extend from the equator to latitude 64° 8′, nd which point they have not yet been looked for; but diminish in amount as the latitude increases. Thus the between ten and eleven P.M., and four and five A.M. is,

In London	0·0162 inch
At Bombay	0·0437
At Poona, (2000 feet high) .	0·0181

he rise from four and five A.M., to nine and ten A.M. is, in

London	0·0185 inch
Bombay	0·0562
Poona	0·0445

he fall from nine and ten A.M., to four and five P.M.,

London	0·0289 inch
Bombay	0·1142
Poona	0·1164

nd the rise from four and five P.M., to ten and eleven P.M.,

London	0·0272 inch

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Bombay	0·0896 inch
Poona	0·0884*

These oscillations are doubtless owing to tides in the atmo sphere, occasioned by the action of the sun. If the height o these tides be proportional to the difference between the speci gravity of air and quicksilver, the morning tide will be abo 13 feet, and the evening tide about 25 feet.

Mean temperature.

The mean temperature of the air at the level of the sea highest at the equator, and gradually sinks as we advance to wards the poles. Professor Mayer, of Göttingen, was o opinion that the mean temperature of any place at the lev of the sea was obtained by multiplying the mean temperatur at the equator by the square of the cosine of the latitude. Bu this empyrical rule fails when we apply it to high latitudes. gives us the mean temperature at the pole 32°, while we a certain from the thermometric tables of Parry and Ross, th latter of whom spent four years in the dreary latitude of 70 at the north-east corner of North America, that the mean tem perature of the pole must be considerably below zero.

Sir David Brewster, in 1820, proposed another empyri rule, which in ordinary latitudes approaches pretty near t truth. It is this. Multiply the mean heat at the equator the cosine of the latitude.

Humboldt has shown by numerous observations, that t mean temperature at the equator and on a level with the s is 81°·5. The mean temperature of latitude 45°, is 5 Now Brewster's formula gives 57°$\frac{1}{3}$, or within a degree an half of the truth. But this formula gives the mean tempe ture of north latitude 70°, 17°·58; whereas, the mean te perature deduced from a series of observations, kept by John Ross every two hours for three years, gives the me temperature of that latitude and west longitude 96° only 0·8 or not quite one degree above zero.

The mean temperature at Paris in latitude 48° 49', i 51°·4796;† that of London about 50°; that of Glasgow 47°·7

* Sykes, Phil. Trans. 1835, p. 161.

† Determined by Bouvard from the maxima and minima for 28 years from 1806 to 1834. See Poisson's *Theorie Mathematique de la Chaleu*

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At Geneva, situated about 1640 feet above the level of the sea, but in latitude 46° 20′ N. the mean temperature is 49° 55′. The temperature at 9h. 30′ A.M., is very nearly the mean temperature of the day.

The range of the thermometer increases with the latitude and with the height above the sea.

At Poona . .	53°·4,	or from	40°·5 to	93°·9
Paris . .	76·2,	.	23·4	99·6
Berne . .	119·25,	.	—24	95·25
St Petersburg .	127·1,	.	—35·7	91·4
Victoria harbour*	130,	.	—60	70

But in Great Britain this rule does not hold, our heat and cold being both checked by the proximity of the sea. The greatest range of the thermometer is,

In London	93° from	0° to	93°
Glasgow	84	0	84

The mean temperature of the table-land in India is much higher than it ought to be from calculation. At Poona, at an elevation of 2000 feet above the sea, the mean temperature is 77°·7.

On the west coast of Europe, the mean temperature is considerably higher than at the same latitude on the east coast of America or Asia.

	Latitude.		
At Enontekis .	67°	it is . .	32°
Table-bay, Labrador	54		
Stockholm .	60	. .	41
Newfoundland .	48		
Belgium . .	51	. .	50
Boston . .	42 30′		

p. 463. The mean height of the thermometer in the cave below the Observatory at Paris, 92 feet under the surface, is 53°·31 from a mean of 352 observations, made from 1st July, 1817, to 18th January, 1835, by M. Bouvard, Ibid. p. 412.

* About N. lat. 70°, where Sir John Ross spent several years.

	Latitude.		
Rome	43°	it is	59°
Raleigh, Carolina	36		
Pekin	39 54′		$55\frac{1}{2}$

In America the winters are colder and the summers warmer than in Europe.

CHAPTER IV.

OF IGNITION.

By *ignition* (perhaps the word *incandescence* would answer better) is meant a property which bodies possess of giving out *light* whenever their temperature is raised to a certain point. When a body is heated up to the temperature of incandescence we say in common language that it is *red hot.*

1. There is reason to believe that all bodies (susceptible of the property) become red hot at exactly the same temperature. But the exact point of incandescence has not been determined. There are two reasons for this. 1. It is higher than the range of a mercurial thermometer. Consequently we are not in possession of an instrument capable of measuring it with precision. 2. But even if we had such an instrument, it would not be easy to determine the point of incandescence, because that point is not well defined. We depend upon the acuteness of our sight to inform us of the beginning of ignition. Now we are not sure that we can perceive the very weakest light. The presumption is rather the contrary, as we know that other animals see objects in such weak light as appears to us to be darkness. And even our eyes will in one condition be affected by a weak light to which they are insensible at another time.

Bodies become red hot at 800°. Sir Isaac Newton calculated the temperature at which a piece of iron begins to shine in the dark to be 635°. He supposed that the heat lost by such an iron, when suspended in a

cold room in a given time, was always proportional to the excess of its heat above that of the surrounding medium. He measured the time which elapsed while the iron was cooling from a red heat till it became cold enough to bear the application of a thermometer, and then observed the rate of its cooling till it reached the temperature of the room. This enabled him to calculate what the original temperature of the iron had been. But this law of Newton, though plausible and seducing, is very different from the real law of cooling. We are sure that the lowest point of ignition is higher than 635°; for the boiling point of mercury is 662°, and boiling mercury is quite invisible in the dark. Mr Daniell measured, by means of his metallic pyrometer, the lowest point at which a heated iron appears red hot in daylight, and found it 980° Fahrenheit. But this must be a good deal higher than the point of ignition of the same iron in the dark. Antimony, when heated to the melting point, appears visibly red hot in the dark. Now, Dr Cromwell Mortimer found the melting point of this metal to be 810°. From an experiment by Sir H. Davy it appears that fusible metal becomes luminous when heated to the temperature of 812°.* The point of ignition is even lower than 810°. We cannot err very far if we place the commencement of ignition at 800°, or between 750° and 800°. Mr Princep has determined, by means of an air thermometer, that a full red heat indicates a temperature of 1200°; and what he calls an orange heat, a temperature of 1650°.

From the experiments of Mr Thomas Wedgewood, we have reason to conclude that all bodies susceptible of the requisite temperature become red hot at exactly the same point. Wood and most liquids cannot be made luminous by heat, because they are dissipated long before they reach the requisite temperature. But metals, stones, and earthy bodies, can all be heated to redness.

Gases do not become red hot.

There is, however, one remarkable exception to this. It does not appear that the gases become luminous even at a much

* Phil. Trans. 1817, p. 53.

Chap. IV. higher temperature. The following ingenious experim Mr T. Wedgewood seems to set the truth of this except a very clear point of view. He took an earthenware bent so in the middle that it could be sunk, and make turns in a large crucible, which was filled with sand. T end of this tube was fixed a pair of bellows; at the oth was a globular vessel, in which was a passage, furnishe a valve to allow air to pass out, but none to enter. was another opening in this globular vessel filled with that one might see what was going on within. The cr was put into a fire; and after the sand had become red h was blown through the earthen tube by means of the be This air, after passing through the red hot sand, came in globular vessel. It did not shine; but when a piece o wire was hung at that part of the vessel where the ea ware tube entered, it became faintly luminous: a proo though the air was not luminous, it had been hot enou raise other bodies to the shining temperature.

M. Saissy concluded from his experiments that oxyge air, and chlorine gas, when suddenly compressed, become nous; but that this is not the case with any other g bodies. Thenard has shown that the light was owing combustion of some matter (oil for example), attached piston, and that when no combustible matter was prese light whatever was evolved by the compression of these g He showed also that when oxygen gas is compressed column of mercury of 102½ inches, a piece of fir placed takes fire and burns brilliantly when heated to 485°; that when the gas is merely subjected to the ordinary spheric pressure, fir cannot be kindled in it though hea 662°.†

Light heats bodies. 2. Thus it appears that whenever a body is heated certain point (about 800°) it immediately begins to emit On the other hand, whenever light is concentrated upo surface of opaque bodies they always become hot. Inde opaque bodies become hotter when they are exposed t

* Ann. de Chim. et de Phys. xliv. 182. † Ibid. p. 184

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direct rays of the sun. The degree of heat induced is influenced by the colour of the body; for *black* bodies, *ceteris paribus*, acquire the *most*, and *white* bodies the *least* heat. When the colour of a body is neither white nor black, the degree of heat induced is proportional to the depth of the shade. Supposing the shades equal, *blue* is the colour that becomes hottest next to black; *green* and *red* follow next after blue; and *yellow* is next to white. The reason seems to be that the intensity of these colours, measured by depth of shade, is in the order in which they have been named. *Blue* may be so intense as to differ but little from *black*. Gadolinite has so deep a shade of green that to the eye it appears almost black. The same remark applies to some varieties of hornblende. Red and yellow are such lively colours that they seldom are so deep as to be mistaken for black.

Heat from the solar rays.

The degree of heat producible by the direct rays of the sun is not easily ascertained; because it is apt to be dissipated as fast as it accumulates. M. de Saussure made a little box lined with fine dry cork, the surface of which was charred to make it black and spongy, in order that it might absorb the greatest possible quantity of the sun's rays, and be as bad a conductor of caloric as possible. It was covered with a thin glass plate. When this box was set in the sun's rays, a thermometer laid in the bottom of it rose in a few minutes to 221°; while the temperature of the atmosphere was only 75°.* Professor Robison constructed an apparatus of the same kind, employing three very thin vessels of flint glass, which transmit more heat than any of the other species of glass. They were of the same shape, arched above, with an interval of $\frac{1}{5}$ inch between them. They were set on a cork base prepared like Saussure's, and placed on down contained in a pasteboard cylinder. With this apparatus the thermometer rose often in a clear summer day to 230°, and once to 237°. Even when set before a bright fire, the thermometer rose to 212°.† But

* Voyages sur les Alpes, ii. 932.

† Black's Lectures, i. 547. When the apparatus was carried to a damp cellar before the glasses were put in their places, so that the air within was moist, the thermometer never rose above 208°; hence Dr Robison

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when its rays are concentrated by a burning-glass, they are capable of setting fire to combustibles with ease, and even of producing a temperature at least as great, if not greater, than what can be procured by the most violent and best conducted fires. In order to produce this effect, however, they must be directed upon some body capable of absorbing and retaining them; for when they are concentrated upon transparent bodies, or upon fluids, mere air, for instance, they produce little or no effect whatever.

Count Rumford has shown by direct experiment, that the heating power of the solar rays is not increased by concentrating them into a focus, but that the intensity of their action is occasioned by a greater number of them being brought to bear upon the same point at once.*

Lavoisier, by means of a powerful burning-glass, not only melted gold, but actually caused it to evaporate; for a piece of silver placed at some height above the gold was sensibly gilded. The utmost heat of a glass-house furnace, acting on gold for a month, produced no sensible evaporation of that metal whatever.

Thus it appears that when heat is accumulated in bodies to a certain amount, it causes them to emit light; and that, on the other hand, when light is accumulated in them, it causes them to become hot. There is indeed one apparent exception to this general law. The light of the moon, though concentrated by the most powerful burning-glasses, is not capable of raising the temperature of the most delicate thermometer. M. de la Hire collected the rays of the full moon when in the meridian by means of a burning-glass 35 inches in diameter, and made them fall upon the bulb of a delicate air thermometer. No effect was produced, though the lunar rays by this glass were concentrated 306 times.† But this is not surprising when we consider the prodigious difference between the intensity of the solar and lunar rays. M. Bouguer, by a very simple and ingenious set of experiments, has shown that the illuminating

Rays of the moon not hot.

concluded, that moist air conducts better than dry; a conclusion fully confirmed by the subsequent experiments of Count Rumford.

* Jour. de Phys. lxi. 32.

† Mem. Paris, 1705, p. 346.

power of a solar ray is 300,000 times greater than that of a lunar ray of the same size.* Indeed, we may satisfy ourselves by a very simple calculation that this estimate of Bouguer is not below the truth. During a part of every lunation, we have an opportunity of seeing the moon in the sky at the same time with the sun. If we compare the moon under such circumstances with a white cloud, when any such happen to be in its neighbourhood, the brightness of the moon and the cloud appear to the eye very nearly identical. So that the moon emits no more light than is emitted during the day by a white cloud of the same visual size. When the whole sky is covered with white clouds, the direct rays of the sun are intercepted; but the light of the day is very nearly the same as if the sky were unclouded and the sun shining. It follows from this that the light emitted by the moon is to that emitted by the sun as the apparent size of the moon to that of the whole sky. Now the moon, when its visual diameter is a maximum, does not cover one-millionth part of the hemisphere of the sky. But let us take Bouguer's measurement, and let us admit that the direct rays of the sun are capable of elevating the thermometer 237°. The rays of the moon would only possess $\frac{1}{300000}$th part of this effect, or $\frac{1}{1250}$th part of a degree. So that even when concentrated 306 times the effect would scarcely amount to $\frac{1}{5}$ of a degree. But I am satisfied that the light of the moon is at least $\frac{1}{1000000}$ of times less intense than that of the sun, so that their heating power, even when concentrated 306 times, could not amount to so much as $\frac{1}{20}$th of a degree.

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Attempts were made to ascertain whether the rays of the moon would produce any effect on the thermo-multiplier, both by Melloni and Professor Forbes, but they could discover no appreciable action. Professor Forbes concentrated the moon's rays 3000 times, and made them fall upon the thermo-multiplier, but the needle made no appreciable motion; the motion certainly did not amount to a quarter of a degree.†

Why do heat and light mutually evolve each other?

3. How comes it that heat and light are mutually disposed to produce each other? Various solutions of this question

* Mem. Paris, 1726, p. 12.

† Phil. Mag. (3d Series) vi. 138.

Chap. IV. have been offered at different times by those philosophers who are fond of speculating on such subjects. It was believed that there exists a mutual repulsion between the particles of light and heat, and hence it was inferred that the accumulation of either would occasion the expulsion of the other. And this was offered as a sufficient explanation of the phenomena. But when we examine appearances somewhat closely, they afford but little to induce us to adopt such an opinion; if they be not (as I think they are) directly inconsistent with it.

The quantity of light given out by bodies always increases with the temperature. At first it is very feeble, then it becomes a dingy red, then a bright red, then yellow or orange, and at last so intense a white that the eye cannot behold it. Now, if we adopt the notion that the light thus emitted existed previously in the luminous body, we must admit that every body in nature possesses the very same quantity of light, and this quantity must be allowed to be inexhaustible; for the light continues as long as we keep up the temperature. An ounce of gold might be kept red hot for a month or a century, and that by communication of heat from a body not itself luminous, as heated air, without any diminution of the intensity of the light which it is constantly emitting. The same remarks must apply to the quantity of heat which all bodies contain; for the heat emitted by them is always proportional to the quantity of light concentrated upon them. So that by powerful burning-glasses, as high a temperature is produced in bodies as can by any other method. Thus, the hypothesis that we are considering supposes that all bodies contain an infinite quantity both of light and heat—a supposition quite inconsistent with the very hypothesis from which it flows.

A second hypothesis supposes that light and heat are absolutely the same thing, the only difference between them being that light is radiated in straight lines through space, while heat exists fixed in bodies. It produces the phenomena of light while it is moving in rays, and the phenomena of heat while it is fixed in bodies; or, according to the undulation theory at present fashionable, the undulations of ether which produce heat, are larger than those which produce light.

This hypothesis accounts tolerably well for the mutual evolution of light by heat, and heat by light; but it is inconsistent with a number of well established facts, which have been already stated while treating of the *radiation of heat*. For nothing is better ascertained than that heat is emitted in straight lines from surfaces.

They seem mutually convertible.

In all probability, light and heat, though not absolutely the same, are yet mutually convertible into each other. Upon what the difference between them depends cannot be explained. Whether they move with different degrees of velocity, or whether it be that the interval between the particles of heat when in motion is too great to produce in us the sensation of vision; it may be that to produce this sensation a succession of particles following each other within a determinate interval of time may be necessary. This difference in the interval may perhaps account for the different kinds of rays which exist in the solar beam. But upon this difficult subject nothing better than conjecture can be offered.

Phosphorescent bodies.

4. There are several bodies which seem to contain light as a constituent; since it can be driven off from them by the application of a moderate heat. The quantity of light which they thus contain is not very great; yet while it is making its escape, the bodies appear luminous, as if they were surrounded by a low lambent flame, varying in colour according to that of the body from which the light is issuing. The minerals which possess the property of emitting light when heated, are called *phosphorescent*. Fluor spar, and phosphate of lime, are the two minerals which possess this property in the greatest perfection. Almost every variety of fluor spar is more or less phosphorescent; but some kinds are much more so than others. The same remark applies to phosphate of lime; the kind which is most phosphorescent of all is the compact variety from Estremadura in Spain. There are some kinds of calcareous spar which likewise phosphoresce; and the *tremolite* seems to owe its phosphorescing quality to the carbonate of lime with which it is usually contaminated.

There are some bodies which seem to contain light as a constituent, and which they appear to part with first, when

Chap. IV. they begin to undergo spontaneous decomposition. this is the case, has been rendered very evident by a experiments made long ago by Mr Canton,* and repea carried a great deal farther by Dr Hulme.† It has be known that different kinds of meat and fish, just wh are beginning to putrefy, become luminous in the dark. course give out light. This is the case in particular the whiting, the herring, and the mackerel. When four d either of these are put into a phial containing two ou sea water, or of pure water holding in solution half a common salt, or two drams of sulphate of magnesia phial be put into a dark place, a luminous ring appears surface of the liquid within three days, and the whole when agitated, becomes luminous, and continues in th for some time. When these liquids are frozen, the li appears, but is again emitted as soon as they are thaw moderate heat increases the luminousness, but a boilin extinguishes it altogether. The light is extinguished water, lime water, water impregnated with carbonic a or sulphuretted hydrogen gas, fermented liquors, sp liquors, acids, alkalies, and water saturated with a va salts, as sal-ammoniac, common salt, sulphate of ma but the light appears again when these solutions are with water. This light produces no sensible effect thermometer.‡ After these experiments, it can scar denied that light constitutes a component part of the stances, and that it is the first of the constituent part makes its escape when the substance containing it is be to be decomposed.

Most bodies absorb light. Father Beccaria, and several other philosopher shown us, by their experiments, that there are a grea substances which become luminous after being expose light.§ This property was discovered by carrying t stantly from the light into a dark place, or by darke

* Phil. Trans. lix. 446. † Ibid. 1800, p. 16

‡ The same experiments succeed with Canton's pyrophoru Hulme has shown.

§ Phil Trans. lxi 212.

chamber in which they are exposed. Most of these substances, indeed, lose their property in a very short time, but they recover it again on being exposed to the light; and this may be repeated as often as we please. We are indebted to Mr Canton for some interesting experiments on this subject, and for discovering a composition which possesses this property in a remarkable degree.* He calcined some common oyster shells in a good coal fire for half-an-hour, and then pounded and sifted the purest part of them. Three parts of this powder were mixed with one part of the flowers of sulphur, and rammed into a crucible which was kept red hot for an hour. The brightest parts of the mixture were then scraped off, and kept for use in a dry phial well stopped.† When this composition is exposed for a few seconds to the light, it becomes sufficiently luminous to enable a person to distinguish the hour on a watch by it. After some time it ceases to shine, but recovers this property on being again exposed to the light.

The only effect which heat had on this pyrophorus was to increase the separation of light from the pyrophorus, and of course to shorten the duration of its luminousness. Two glass globes hermetically sealed, containing each some of this pyrophorus, were exposed to the light and carried into a dark room. One of them, on being immersed in a basin of boiling water, became much brighter than the other, but in ten minutes it ceased to give out light: the other remained visible for more than two hours. After having been kept in the dark for two days, they were both plunged into a basin of hot water; the pyrophorus which had been in the water formerly did not shine, but the other became luminous, and continued to give out light for a considerable time. Neither of them afterwards shone by the application of hot water; but when brought near to an iron heated so as scarcely to be visible in

* Phil. Trans. lviii. 327.

† Dr Higgins has added considerable improvements to the method of preparing Canton's pyrophorus. He stratifies the oyster shells and sulphur in a crucible without pounding them; and after exposing them to the proper heat, they are put into phials furnished with ground stoppers.

the dark, they suddenly gave out their remaining light, and never shone more by the same treatment: but when exposed a second time to the light, they exhibited over again precisely the same phenomena: even a lighted candle and electricity communicated some light to them.

Light from friction.

Many bodies emit light from friction. The way in which the effect is produced in this case is not well understood. It seems to be connected with electricity. The evolution of sparks when flint and steel are struck against each other, is a case of combustion, and does not come under the class of bodies that become luminous by friction.

CHAPTER V.

OF COMBUSTION.

This effect of heat differs from those already treated of in one remarkable and important particular; it is not universal. There are many substances which are not susceptible of undergoing combustion, though subjected to ever so high a temperature,—and there are some bodies which are capable of undergoing combustion without the application of any artificial heat whatever. It is, however, a very general effect of heat, and it deserves to be treated somewhat in detail, because the earliest attempts at generalizing in chemistry were founded upon a theory of combustion, and because the different theories of combustion have had a most striking effect upon the progress and aspect of the science.

Meaning of the term

1. By *combustion* is meant, in common language, a remarkable evolution of *heat* and *light*, or of *fire*, when certain bodies combine together. This is well exemplified in the burning of wood, or pit coal, or oils, or spirits, in the open air. These bodies give out an enormous quantity of heat and light during their combustion. The substance with which they combine

is the oxygen of the atmosphere. When phosphorus is raised to the temperature of 148°, it catches fire, and burns with great splendour, combining with the oxygen of the atmosphere, and giving out a vast quantity of heat and light. Hydrogen gas requires a red heat to cause it to begin to burn. It then unites rapidly with the oxygen of the atmosphere, and gives out little light, but a great quantity of heat.

In all these cases of combustion we only see one of the substances which combine. The oxygen of the atmosphere, though just as essential an ingredient, is not visible, and its agency was long unknown. This is the reason why the wood, the pit coal, the oil, the phosphorus, the hydrogen, has received, in common language, the name of *combustible.* As both the bodies commonly called combustibles, and the oxygen of the atmosphere, are essentially necessary for combustion,—as during the combustion they unite together, and as the evolution of light and heat, which constitutes the most striking part of the phenomenon, are the immediate consequence of this union, it is evident that in strict propriety of language the oxygen is as well entitled to the name of combustible as the other body with which it unites. But as common language was fixed before the nature of the process of combustion was understood, the term *combustible* came to be applied to one of the two substances which unite together, and not to the other; to that, namely, which is the most conspicuous. It would be in vain for us to attempt to alter the common language, and to affirm that the oxygen is just as much a combustible as the wood or the pit coal which we burn on our fires. We would never be able to prevail upon mankind to adopt our nomenclature. It will be better for us then to apply the term *combustible*, as it is done in common life, and we may distinguish the other constituent by the name *supporter of combustion.*

Combustible, what.

Supporter, what.

Combustion, then, is a union of a *combustible* with a *supporter*, attended with the evolution of *heat* and *light*, or of *fire.*

Combination occasions a change of temperature.

2. No chemical combination of two bodies with each other ever takes place without a change of temperature. Sometimes the temperature sinks, as when sulphate of soda is dissolved in a dilute mineral acid, or when snow and common salt are mixed together. Cold is produced only when solid

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bodies become liquid by uniting together; and the more rapidly they liquefy, the greater is the intensity of the cold produced. In the greater number of chemical combinations heat is evolved, and the more rapidly the combination takes place, and the more intimate it is, the greater is the evolution of heat. Those combinations which are accompanied by the evolution of heat, always undergo an increase of density, or the particles constituting them approach nearer each other than they were before. An example will make this approach of the particles obvious.

Usually an increase.

Water is a compound of

2 volumes hydrogen gas
1 volume oxygen gas

united together and condensed into a liquid.

Sp. gravity of hydrogen gas 0·0694
oxygen gas 1·1111

that of common air being unity.

	Grains.
2 cubic inches of hydrogen gas at 32° weigh	0·045512
1 cubic inch of oxygen gas weighs	0·364330
	0·409842
Weight of 3 cubic inches of the constituents of water before combination	0·409842
Weight of 3 cubic inches of water	757·28

And why.

Now, the volumes before and after combination are inverse as these weights. From this it follows that 1700 cubic inches of the constituents of water, when they combine together and assume the form of water, occupy little more than 1 cubic inch. So that the particles approach very nearly 12 times nearer each other than when they were in the gaseous state. This is an enormous condensation. Now, the heat evolved during the rapid union of oxygen and hydrogen gases is the greatest that we have it in our power to produce.

Combustion only follows rapid combination.

3. It is only when bodies combine with each other rapidly that their combination is accompanied by combustion. When the combination takes place slowly, the total quantity of heat evolved may be the same as in the rapid combination; but as

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it is evolved slowly, it is dissipated as it appears, and never accumulates in sufficient quantity to constitute *fire*, which always makes its appearance when heat is accumulated in a body to a certain amount. This is probably the reason why combustion is confined to cases of rapid combination.

Number of supporters.

4. In all cases of combustion which occur in common life, the supporter of combustion, which combines with the burning body, is the oxygen of the atmosphere. But there are three other bodies which possess analogous properties to oxygen. They accumulate, like it, round the positive pole of the galvanic battery. Like it, they combine with almost all the simple bodies, and form, with them, compounds of an analogous nature. These three bodies are *chlorine*, *bromine*, and *iodine*; the first of which is a gas like oxygen, the second a liquid, and the third a solid. These four bodies, from the analogy of their properties, may be conveniently classed together, and they may be distinguished by the name of *supporters of combustion*. Of these *chlorine* is the best, and *iodine* the worst supporter; probably owing to the state of their aggregation. The similarity of chlorine, bromine, and iodine, to each other, is much greater than their analogy with oxygen, which they resemble chiefly in their property, like it, of combining with all simple bodies, and of acting occasionally as supporters of combustion.

Sulphur and phosphorus occasionally act the parts of supporters. When sulphur, in a liquid state, is made to combine rapidly with copper or zinc or iron, and perhaps also with some other bodies, it becomes solid in the instant of union, and the new compound becomes red hot, and exhibits all the phenomena of a short combustion. When liquid phosphorus is brought in contact with hot lime, barytes, or strontian, a rapid combination takes place, and all the phenomena of a brilliant combustion present themselves. In this last case the oxygen previously united with the calcium, barium, and strontium, suddenly enters into combination with the phosphorus, and may contribute to the combustion; but probably the greatest share of the effect is due to the sudden change of the phosphorus from a liquid to a solid state.

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Potassium also frequently produces the phenomena of combustion, and may therefore be considered as an occasional supporter; and doubtless sodium and the metallic bases of the alkaline earths would show similar properties if we had it in our power to procure them. In all these cases, the potassium is liquid, and suddenly changes by combining with oxygen or with another metal into a solid body.

Number of combustibles.

The combustible bodies consist of all the simple substances with the exception of the four supporters. There exist likewise some striking anomalies among the simple bodies, in the phenomena of their union with the supporters. But it will be better to leave the consideration of these phenomena, till we come to treat of those bodies themselves in which they occur.

Glowing explained.

5. There is another set of phenomena which have been considered as connected with combustion; but which I rather think should be classed as instances of phosphorescence. When the green oxide of chromium is heated nearly to redness, it becomes of itself red hot, and glows for some time like a live coal. By this glowing it undergoes no alteration in its weight; but becomes much more difficultly soluble in acids than formerly, and its specific gravity is probably increased. This glowing does not always take place; though it is a very common property of the oxide. No doubt the presence or the absence of the property depends upon the mode of preparing the oxide. I consider the light that escapes in this example as having been chemically combined, and adhering so strongly, that a pretty high temperature is requisite to drive it off. In like manner, when the mineral called *gadolinite* is exposed to a heat very nearly amounting to a red heat, it becomes red hot and glows for a little time with considerable brilliancy. This also I consider as a case of phosphorescence.

What shows the agreement of these phenomena with the phosphorescence of fluor spar and phosphate of lime is, that though no sensible alteration is made in the weight or chemical constitution of the substances, they cannot be made to glow a second time by exposure to heat. The green oxide of chromium may indeed have its property of glowing restored by

heating it with carbonate of potash or soda, which converts it into chromic acid; digesting the chromate thus formed with oxalic acid, which reduces the acid to green oxide, and then throwing it down by ammonia. But fluor spar and phosphate of lime recover their phosphorescing quality when decomposed and again united, as was ascertained by Scheele; so that those bodies which phosphoresce, and those which glow, resemble each other in every part of the phenomenon.

Quantity of heat evolved during combustion.

6. To estimate the quantity of heat evolved during the burning of different combustibles is not only important in a philosophical point of view, but of considerable consequence also as an object of economy. A set of experiments on this subject was made by Lavoisier and Laplace. They burnt various bodies in the calorimeter, and estimated the heat evolved by the quantity of ice melted in each experiment. Dr Crawford made a similar set of experiments. He estimated the heat evolved by the increase of temperature which the water experienced with which he contrived to surround the burning bodies.* A still more numerous set of experiments has been made by Mr Dalton, chiefly on the heat evolved during the combustion of gaseous bodies. He filled a bladder capable of holding 30,000 grains of water with the gas; this bladder was fitted with a stop-cock and a pipe. A tinned vessel was procured capable of holding 30,000 grains of water; the specific heat of which being ascertained, and as much water added as made the specific heat of both equivalent to that of 30,000 grains of water, the gas was squeezed out of the bladder, lighted, and the extremity of the flame made to play upon the bottom of the tinned vessel. The quantity of heat evolved was estimated by the increase of temperature produced upon the water in the vessel.†

A very numerous set of experiments was likewise made by Count Rumford on the heat evolved during the combustion of oils, spirits, and various woods; and from the length of time

* See his Experiments on Animal Heat, pp. 254, 320, 333.

† Dalton's New System of Chemical Philosophy, p. 76. When the substances burnt were not gaseous, they were converted into a lamp and burnt with or without a wick as the case required.

Chap. V. which he devoted to the subject, and the numerous prec to which he had recourse, it is probable that his resu near approximations to the truth.*

These different experiments cannot well be compare ther, in consequence of the great difference in the m conducting them. In Dalton's experiments a good deal heat would of necessity be lost, yet as all the gases were as nearly as possible in the same circumstances, they o give us, though not the absolute heat evolved, yet the r quantity given out during the combustion of each of th which he employed. I shall therefore give them in the ing table:—

Experiments of Dalton.

Substances burnt 1 lb.	Oxygen consumed in lbs.	Ice melted in lbs.	Rat
Hydrogen gas	8	320	
Carburetted hydrogen	6	85	
Olefiant gas	4·375	88	
Carbonic oxide	0·4375	25	
Olive oil	3·033	104	
Wax	3·14	104	
Tallow	—	104	
Oil of turpentine	—	60	
Alcohol	3·477	58	
Sulphuric ether	4·296	62	
Phosphorus	1·333	60	
Charcoal	2·666	40	
Sulphur	1·5	20	
Camphor	3·021	70	
Caoutchouc	5·265	42	

In these experiments, the greatest loss of heat w sustained in the burning of the charcoal, because it d give out flame. Camphor and oil of turpentine would underrated, because much of their carbon flies off unb the form of lamp black.

Of Davy. Sir Humphry Davy, in his very curious and inte researches on flame, has given us the results of a set of

* Nicholson's Journal, xxxii. 105; xxxiv. 319, and xxxv. 95.—(Annalen, xlv. 1, 300.

ments on the heat produced by the combustion of five different gases. The gases were made to issue from the mouth of a platinum tube, and the jet being inflamed was made to play upon the bottom of a copper vessel containing oil, raised previously to the temperature of 212°. The bulk of each gas consumed, and the time of burning, were in each case as nearly as possible the same. The following are the results:—*

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6	.	Olefiant gas heated the oil to	270°	or 58°
1	.	Hydrogen . . .	238	26
4	.	Coal gas . . .	236	24
3	.	Sulphuretted hydrogen .	232	20
1	.	Carbonic oxide . .	218	6

The numbers in the first column give the relative quantities of oxygen consumed during these combustions. These results cannot well be compared with those in the preceding table. It is obvious that much heat would be dissipated during the continuance of the experiment, in consequence of the previously high temperature of the oil.

It is not easy to see why the oil was previously heated to 212°. It could have no other effect than that of rendering the loss of heat greater than it otherwise would be.

Of Lavoisier, Crawford, Dalton, and Rumford.

The following table exhibits the result of the experiments of Lavoisier, Crawford, Dalton, and Rumford:—

Substances burnt 1 lb.	Oxygen consumed in lbs.	Ice melted in lbs.	Experimenter.
Hydrogen	8	295	Lavoisier
		480	Crawford
		320	Dalton
Charcoal	2·66	96·5	Lavoisier
		69	Crawford
		40	Dalton
Phosphorus	1·33	100	Lavoisier
Olive oil	3·033	148	Lavoisier
		89	Crawford
		104	Dalton
		93·073	Rumford
Rape oil	3·033	124·097	Rumford

* Phil. Trans. 1817, p. 52.

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Substances burnt 1 lb.		Oxygen consumed in lbs.		Ice melted in lbs.		Exper
Wax	.	3·14	.	133	.	Lav
				97	.	Cra
				104	.	Dal
				126·242	.	Rum
Tallow	.	—	.	104	.	Dal
				111·582	.	Rum
Alcohol	.	3·477	.	67·47	.	Rum
Sulphuric ether		4·296	.	107·027	.	Rum
Naphtha	.	—	.	97·834	.	Rur

The result of Count Rumford's experiments on th bustion of woods will be seen in the following table :—

Species of wood.	Quality.	l...
Lime	Joiner's dry wood, 4 years old	
Ditto	Ditto	
Ditto	Ditto, highly dried over a chafing dish	
Ditto	Ditto	
Ditto	Ditto, rather less dried	
Beech	Joiner's dry wood, four or five years old	
Ditto	Ditto	
Ditto	Ditto, highly dried over a chafing dish	
Ditto	Ditto	
Elm	Joiner's wood, rather moist	
Ditto	Joiner's dry wood, four or five years old	
Ditto	Ditto	
Ditto	Ditto, highly dried over a chafing dish	
Ditto	Ditto	
Ditto	Ditto, dried and scorched in the stove	
Oak	Common fire wood in moderate shavings	
Ditto	Ditto, in thicker shavings, leaving a residuum of charcoal	
Ditto	Ditto, in thin shavings	
Ditto	Ditto, in thin shavings, well dried in the air	
Ditto	Joiner's wood, very dry, in thin shavings	
Ditto	Ditto	
Ditto	Thick shavings, leaving 0·92 grains of charcoal	
Ash	Joiner's common dry wood	
Ditto	Same kind, shavings dried in the air	
Ditto	Ditto, highly dried over a chafing dish	
Maple	Seasoned wood, highly dried over a chafing dish	
Service	Ditto, ditto	
Ditto	Same kind, scorched in a stove	
Cherry	Joiner's dry wood	
Ditto	Same kind, highly dried over a chafing dish	
Ditto	Same kind, scorched in a stove	
Fir	Joiner's common dry wood	

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Species of wood.	Quality.		Ice in lbs. melted by the heat developed during the burning of a lb. of the combustible.
Fir . .	Shavings well dried in air		45·333
Ditto . .	Highly dried over a chafing dish		49·838
Ditto . .	Dried and scorched in a stove		44·477
Ditto . .	Thick shavings, leaving much charcoal		38·260
Poplar . .	Joiner's common dry wood		46·134
Ditto . .	Same kind, highly dried over a chafing dish		49·548
Hornbeam	Joiner's dry wood		42·400
Ditto . .	Ditto		42·145
Oak . .	With 19·6 per cent. of water imperfectly burnt, leaving in the combustion a charcoal residuum amounting to	0·81 gramme	35·2:8
		0·73	34·121
		0·94	34·556

Conclusions.

It is not very easy to draw any conclusion from these experiments. The enormous differences between the results obtained by Lavoisier, Crawford, and Dalton, when they operated upon the same substance, destroys all reliance upon the accuracy of the experiments. The most important of all the substances tried was hydrogen and charcoal, because they constitute almost the only combustible constituents of the substances commonly employed for the purpose of fuel.

Now, from Mr Dalton's mode of experimenting, it is clear that his result must have been below the truth. It is evident therefore that Lavoisier's statement of the quantity of heat evolved during the combustion of hydrogen is greatly below the truth. There are two inaccuracies in the calculations employed by Dr Crawford. He overrated the specific gravity of hydrogen gas, and likewise its specific heat. These occasioned an error in his estimate in excess. I think it probable, that we will not be far from the truth if we say, that 1 lb. of hydrogen gas, while burning, gives out as much heat as would melt 400 lbs. of ice, which is equivalent to 56,000 degrees; or it would raise the temperature of 1 lb. of water 56,000 degrees.

All the experimenters would be likely to underrate the heat evolved during the combustion of charcoal. I think therefore that we may estimate the heat evolved during the combustion of 1 lb. of charcoal, as sufficient to melt 100 lbs. of ice. This is equivalent to 14,000 degrees of heat, or it would raise the temperature of 1 lb. of water 14,000 degrees.

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But the quantity of oxygen consumed during the combustion of a lb. of hydrogen and a lb. of charcoal is very different. The hydrogen requires 8 times its weight of oxygen, while the charcoal requires only 2·66 times its weight of that principle: hence it is obvious, provided our estimates be tolerable approximations to the truth, that the heat evolved during the union of 1 lb. of oxygen with hydrogen, would melt 50 lbs. of ice, while the heat evolved during the union of 1 lb. of oxygen with carbon would melt only 37·6 lbs. of ice. Thus it appears that the heat evolved during the burning of hydrogen and charcoal, is not equal whether we compare the action during the union of equal weights of oxygen, or of the combustibles themselves.

The results would approach somewhat nearer equality, if we were to compare the atomic weights of each, which seems to be the most reasonable way of considering the subject. The atom of carbon is 6 times as heavy as that of hydrogen. But during its combustion it combines with two atoms of oxygen, while hydrogen unites only with one atom. To compare the effect of the union of the same number of atoms of oxygen with hydrogen and carbon, we should multiply the heat from the combustion of the same weight of carbon as hydrogen, by 3. It is obvious that when the same number of atoms of oxygen unite with hydrogen and with carbon, the heats evolved in both cases are to each other, as 4 to 3. If the heat be reckoned 4 when the hydrogen combines with oxygen, it will be only 3 when the carbon unites with oxygen. If Lavoisier's experiments on the combustion of phosphorus be correct, the heat which it gives out is double that given out by carbon.

Ratios of the heats evolved.

So that when the same number of atoms of hydrogen, carbon, and phosphorus, unite each with one atom of oxygen, the heats given out are as follows:—

Phosphorus	6
Hydrogen	4
Carbon	3

An accurate set of experiments on this subject, could not fail to give us many interesting and highly useful results.

Indeed a set of experiments has been lately made by M. Despretz. But he has merely published the results, without entering into any details. It is therefore impossible to know how far his results can be depended on. According to him, when one gramme of oxygen combines by combustion with the following bodies, the heat evolved is

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For Hydrogen	. .	2578° centigrade
Carbon	. . .	2967
Iron	. . .	5325

Phosphorus, zinc, and tin, disengage nearly the same quantity of heat that iron does.*

Order in which bodies begin to burn.

7. Every substance seems to have a temperature of its own, at which it is capable of burning; and it never burns till it is raised to the requisite temperature. The following is the order of combustibility of different bodies, according to the observations of Davy, beginning with the body which burns at the lowest temperature:—†

Phosphorus
Phosphuretted hydrogen gas
Hydrogen and chlorine
Sulphur
Hydrogen and oxygen
Olefiant gas
Sulphuretted hydrogen
Alcohol
Wax
Carbonic oxide
Carburetted hydrogen.

Thus it appears that carburetted hydrogen is the gas which requires the highest temperature, and phosphuretted hydrogen the lowest temperature to maintain its combustion.

* Ann. de Chim. et de Phys. xxxvii. 180. The ratios of the heat given out when one atom of these bodies unites with one atom of oxygen are as follows:—

Hydrogen	. . . .	5
Carbon	. . . .	5·75
Iron	. . . .	10·25

† Phil. Trans. 1817, p. 48.

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When these bodies are burnt in rarefied air, the heat evolved is less, because a smaller quantity burns at once. And whenever the temperature produced is not sufficient to maintain the combustion, the flame is extinguished.* Hence, phosphorus will burn in air rarefied 60 times, while carburetted hydrogen will not burn in air rarefied four times.

When gases are rarefied by heat, their combustibility, as appears from Davy's experiments, is increased instead of being diminished as Grotthus supposed.

Effect of gases in preventing explosion.

When a mixture of two volumes hydrogen gas and one volume oxygen gas is diluted with determinate quantities of other gases, it loses its property of exploding when an electric spark is passed through it. The following table exhibits the volumes of the different gases which destroyed the explosive property of one volume of such a mixture, according to the experiments of Davy† and Henry.‡

	Volumes.
Hydrogen	8
*Azote	6
Oxygen	9
Protoxide of azote	11
*Cyanogen	1·5
Carburetted hydrogen	1
*Carbonic oxide	4
Sulphuretted hydrogen	2
Olefiant gas	0·5
Muriatic acid gas	2
Fluosilicic acid gas	0·83
*Ammonia	2
*Carbonic acid	3

Inflammation took place when the volumes of these gases present were as follows:—

	Volumes.
Hydrogen	6
Oxygen	7

* Davy, Phil. Trans. 1817, p. 48. † Ibid. p. 59.

‡ Ibid. 1824, p. 282. Those marked thus (*) by Henry, the others by Davy.

	Volumes.
Protoxide of azote	10
Carburetted hydrogen	0·75
Sulphuretted hydrogen	1·5
Olefiant gas	0·33
Muriatic acid gas	1·5
Fluosilicic acid gas	0·75

Davy is of opinion that the reason why such mixtures prevent the inflammation of the hydrogen, is, that they serve to carry off the heat, or to prevent it from reaching the requisite point.

Nature of flame.

8. Flame is the rapid combustion of volatilized matter. The tallow or the wax is melted and drawn up to the top of the wick of a candle. Here it is boiled and converted into vapour, which ascends in the form of a column. This vapour is raised to such a temperature, that it combines rapidly with the oxygen of the surrounding atmosphere, and the heat evolved is such as to heat the vapour to whiteness. Flame then is merely volatile combustible matter heated white hot. The combustion can only take place in that part of the column of hot vapour that is in contact with the atmosphere, namely, the exterior surface. The flame of a candle then is merely a thin film of white hot vapour, enclosing within a quantity of hot vapour which, for want of oxygen, is incapable of burning. But as it advances upward in consequence of the outer film being already consumed, it gradually constitutes the outer surface of the column, and assumes the form of flame. And as the supply of hot vapour diminishes as it ascends, and at last fails altogether, the flame of a candle gradually tapers to a point. That this is the nature of flame has been beautifully shown by my late friend Mr Oswald Sym, in a paper which has been greatly admired, but which has not perhaps attracted all the attention which it deserves.* Mr Davies, in a very interesting paper, has fully confirmed the accuracy of Mr Sym's observations.†

9. There are several vapours which burn at a lower tem-

* Annals of Philosophy, (1st series,) viii. 321.

† Ibid. (2d series,) x. 447.

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Platinum coil maintains the combustion of alcohol vapour.

perature than that which produces flame. During the bustion heat is evolved; but the ultimate products are different from those formed, when the same vapours burn flame. Sir H. Davy discovered that if a coil of platinum of rather a small size, be placed round the wick of a of wine lamp, while the alcohol is burning, and allowed t main till it gets red hot, we may blow out the flame wit putting an end to the combustion. The wire continues hot, the vapour continues to rise and undergoes a slow bustion round it, during which it gives out as much he keeps the wire always at a red heat. This combustion wit flame goes on as long as any spirit remains to evaporate, it may be kept up for weeks without intermission. alcohol is converted into acetic acid, and a substance ha a peculiar smell. No doubt water is also formed. The at constituents of alcohol are,

3 atoms hydrogen
2 atoms carbon
1 atom oxygen.

While the atomic constituents of acetic acid are

3 atoms hydrogen
4 atoms carbon
3 atoms oxygen.

Were we to suppose two atoms of alcohol during this combustion to unite with 4 atoms of oxygen: they mig converted into 1 atom acetic acid and 3 atoms water. Fo atoms alcohol contain,

	Hydrogen	Carbon.	Oxyg
	6 atoms	+ 4 atoms	+ 2 at
Adding of oxygen	.	. .	. 4
Total	6	+ 4	+ 6
1 atom acetic acid contains	3	+ 4	+ 3
3 atoms water contain	3	+ 0	+ 3
	6	+ 4	+ 6

But it is obvious from the peculiar smell of the acet

formed, that the substances generated are more numerous than acetic acid and water. Chap. V.

The vapour of ether, camphor, and several of the volatile oils, may be made to undergo this combustion without flame as well as alcohol, and the phenomena are nearly the same.

Palladium wire has been substituted for platinum; but when wires of other metals are employed the process does not succeed.

If a coil of small platinum wire, previously heated to redness, be let down into a glass containing a mixture of coal gas and air, or vapour of ether and air, it will continue red hot till the whole gas or vapour be consumed.

Causes of this.

This curious process seems to depend upon two properties which platinum possesses. Its specific heat is very small, and it is one of the worst conductors of heat of all the metals: hence a comparatively small quantity of heat is sufficient to make it red hot, and being a bad conductor, the waste of heat is small compared to what it would be if silver or gold (which are very good conductors) were employed. Red hot platinum is sufficiently hot to cause the rapid union of alcohol vapour with oxygen, but not sufficiently so to cause it to burn with flame.

Spongy platinum, &c. becomes red hot when in contact with alcohol vapour.

10. In the year 1822, M. Dobereiner, professor at Jena, announced that if spongy platinum, obtained by exposing ammonia-muriate of platinum to a low red heat, be heated and put into the vapour of alcohol, it becomes red hot and remains so as long as any of the vapour is unconsumed.* He found that powdered black oxide of manganese, oxide of nickel, oxide of cobalt, oxide of uranium, oxide of tin, &c. when in the loose and porous state in which they are procured by decomposing the oxalates of the respective metals by heat in the open air, may be substituted for spongy platinum. So that the phenomenon is much more general than was at first supposed.

Or by a jet of hydrogen.

In the year 1823, Dobereiner discovered that if a jet of hydrogen gas be made to strike against a small collection of this spongy platinum, at the distance of an inch or two from

* Schweigger's Jahrbuch, iv. 91.

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the mouth of the jet, the platinum becomes red hot, and continues so as long as the hydrogen jet plays upon it. Or if a mixture of oxygen and hydrogen gases be put into a glass jar, and a little spongy platinum be let up into it; the platinum speedily becomes red hot and causes the mixture of gases to explode.* This curious experiment was almost immediately repeated and verified in every part of Europe.

Mr Garden found that the black matter which remains behind when native platina is dissolved in aqua regia, and which contains a good deal of osmium and iridium, may be substituted for the spongy platinum.† It is only necessary to heat it to redness and allow it to cool before using it. Thenard and Dulong found that palladium, rhodium, and iridium, might be substituted for the spongy platinum. Osmium requires to be heated to 104° or 122°. Nickel in a spongy state acts also, but very slowly.‡ Platinum foil in its natural state would not do, but when crumpled like paper it became red hot like spongy platinum.§

When the temperature is elevated, but not so high as that of boiling mercury, all the metals have a greater or smaller effect when treated with a jet of hydrogen gas. Gold in plates requires to be heated to 496°, when in foil to 406°, but when in powder it causes the combination of oxygen and hydrogen gas, if it be heated to 248°.

Also other bodies.

Even charcoal, pumice, porcelain, glass, and rock crystal, determine the combination of oxygen and hydrogen gases, at a temperature below 662°.‖ When the fragments of glass employed were angular they produced twice the effect of the same quantity of glass, consisting of rounded fragments.

Thenard and Dulong found that platinum, in all states, and likewise all the other bodies, gradually lose this property of becoming incandescent, by exposure to the air. But the property may be restored again at pleasure, simply by heating

* Schweigger's Jahrbuch, viii. 321.
† Annals of Philosophy, (2d series) vi. 466.
‡ Ann. de Chim. et de Phys. xxiv. 380.
§ Ibid. xxiii. 440.
‖ Ibid. xxiv. 381.

the bodies red hot, and allowing them to cool; or by plunging them into nitric acid, either cold or hot, allowing them to remain for some time in it, then drying them in a heat of about 390°. The acid may now be washed off with water, or even with an alkaline ley, without depriving the spongy mass of its property. Concentrated sulphuric acid, or muriatic acid, may be substituted for nitric acid, but they do not answer so well.*

Theory of this.

The theory of this curious process seems connected with the capillary attraction for oxygen gas of the small pores in the spongy platinum. It has been proved by the experiments of Count Morozzo, and M. de Saussure, junior, that the pores of charcoal, and other porous bodies, have the property of absorbing certain gaseous substances so copiously, that they exist within the pores in a condensed state. Thus charcoal absorbs $9\frac{1}{4}$ times its own bulk of oxygen gas. We may admit that the oxygen of the atmosphere is condensed to this amount in spongy platinum. Count Morozzo found that when charcoal thus impregnated with oxygen gas is placed in contact with hydrogen gas, the two gases combine, and water is formed. We may admit that when the hydrogen gas strikes the surface of the spongy platinum, combination takes place, and some water is formed. This occasions the evolution of heat, which elevates the temperature of the spongy platinum in consequence of its small capacity for heat, and its imperfect conducting power. This elevation occasions the formation of water in that part of the jet which strikes the platinum, as it consists of a mixture of hydrogen and common air. Thus, more heat is evolved, the platinum becomes red hot, and the combustion goes on precisely, as when the red hot platinum coil is left in contact with the vapour of alcohol. When the spongy platinum loses its property of becoming red hot, the reason probably is that it has lost the property of condensing oxygen gas in its pores. The ignition, or the action of nitric acid, would seem to restore this property.

Platinum with clay occasions the combination of oxygen and hydrogen.

11. Dobereiner found that when a little spongy platinum was made up into a ball with pipe-clay of about the size of a

* Ann. de Chim. et de Phys. xxiv. 383.

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pea, and baked in an incipient red heat, to make the whole adhere together, these balls possessed a similar action upon a mixture of oxygen and hydrogen gases, as the spongy platinum itself. When let up into a jar containing a mixture of oxygen and hydrogen gases in the requisite proportions, the two gases combine with great rapidity, and in a few minutes, and are converted into water. A highly valuable and instructive set of experiments on the use of these platinum balls in the analysis of combustible gases, has been made by Dr Henry.*

He confirmed the results obtained by Dobereiner, by letting up a platinum ball (previously heated to redness by the blow-pipe, and just allowed to cool) into an explosive mixture of 2 volumes of hydrogen gas and one volume of oxygen.

Its action on carbonic oxide.

When carbonic oxide gas is mixed with this explosive mixture, and a platinum ball let up, the carbonic oxide is first acted on, and converted into carbonic acid. If the quantity of oxygen be sufficient, the hydrogen also is converted into water, and an explosion almost always takes place.

On olefiant gas.

When olefiant gas is mixed with the explosive mixture, and in equal volumes with it, and a platinum ball is let up, the explosive mixture is rapidly converted into water, while not above $\frac{1}{10}$th or $\frac{1}{8}$th of the olefiant gas is consumed. The action on the olefiant gas is greater when the explosive mixture exceeds the volume of the olefiant gas. Thus, when $2\frac{1}{4}$ volumes of the explosive mixture is mixed with 1 volume of olefiant gas, about $\frac{1}{4}$th of the olefiant gas is consumed. When the oxygen gas present is sufficient to saturate both the hydrogen and olefiant gas, then in general the action is much more rapid, and both gases are consumed.

On carburetted hydrogen.

When carburetted hydrogen gas is added to the explosive mixture, and a platinum ball is let up, the hydrogen and oxygen combine rapidly into water, but the carburetted hydrogen is not acted on at all, unless the quantity of oxygen gas present be very considerable, and even then the action is very imperfect.

When we make a mixture of carbonic oxide, carburetted

* Phil. Trans. 1824, p. 266.

hydrogen, and oxygen gases, and let up a platinum ball, the carbonic oxide is converted into carbonic acid, but the carburetted hydrogen remains unacted on. Chap. V.

When the mixture consists of

Hydrogen
Carbonic oxide
Carburetted hydrogen
Oxygen,

the two first gases combine with oxygen, and undergo combustion, but the carburetted hydrogen remains unaltered. If olefiant gas be present, it is also partly consumed.

When these four inflammable gases are mixed together with oxygen, and exposed to the action of the platinum ball, the carbonic oxide is first converted into carbonic acid, then the hydrogen is converted into water. The olefiant gas is next acted on, but only imperfectly, and the carburetted hydrogen is not acted on at all. Method of analyzing mixtures of these gases.

This order obviously depends upon the temperature necessary to occasion the combustion of each of these gases. Davy ascertained that hydrogen gas and oxygen combine silently, and form water at a temperature between 662° and 800°—that carbonic oxide is as easily consumed as hydrogen—that olefiant gas requires a red heat—and carburetted hydrogen a white heat, to cause them to burn.

Dr Henry ascertained that when a platinum ball is let up into a mixture of carbonic oxide gas and oxygen, the two gases begin to unite slowly when heated to the temperature of 300°, and they unite rapidly at 340°.

Olefiant gas and oxygen begin to combine at 480°, and are slowly, but completely, consumed at 520°. But a mixture of carburetted hydrogen and oxygen is not in the least acted on when heated to 555°. Nor is a mixture of cyanogen and oxygen acted on though heated to the same temperature.

A mixture of muriatic acid and oxygen gas, begins to be acted on by the platinum ball, and water to be formed, when the temperature is raised to 250°, and a mixture of ammonia and oxygen at the temperature of 380°.

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The following table shows the effect of various gases, in preventing the action of the platinum ball on an explosive mixture of oxygen and hydrogen gases, constituting 1 volume according to the experiments of Dr Henry:—

Not prevented by many volumes of	Hydrogen
———————————	Azote
——— - - by 10 volumes of	Oxygen
———————————	Protoxide of Azote
Prevented by 1 volume of	Cyanogen
Not prevented by 10 volumes of	Carburetted hydrogen
Prevented by ½ volume of	Carbonic oxide
Prevented by 1·5 volume of	Olefiant gas
Not prevented by 6 volumes of	Muriatic acid
——————— 10 volumes of	Ammonia
———————————	Carbonic acid.

Dr Henry has ingeniously applied these facts to the analysis of mixtures of

Olefiant gas
Hydrogen
Carbonic oxide
Carburetted hydrogen.

The olefiant gas is removed by chlorine, and its volume determined. Then oxygen gas is mixed with the mixture of the three remaining gases, and a platinum ball let up. The carbonic oxide is converted into carbonic acid, and the hydrogen into water. Caustic potash, by absorbing the carbonic acid, determines the volume of carbonic oxide. Two-thirds of the diminution of volume (subtracting the carbonic oxide) is the volume of hydrogen. Nothing remains but the carburetted hydrogen, which, being mixed with oxygen gas in the requisite proportion, is fired by electricity, and its quantity determined in the usual way.

First explanation of combustion.

12. The first attempt to explain combustion was crude and unsatisfactory. A certain elementary body, called *fire*, was supposed to exist, possessed of the property of devouring certain other bodies, and converting them into itself. When we set fire to a grate full of charcoal, we bring, according to this

hypothesis, a small portion of the element of fire, which immediately begins to devour the charcoal, and to convert it into fire. Whatever part of the charcoal is not fit for being the food of fire is left behind in the form of ashes. Chap. V.

A much more ingenious and satisfactory hypothesis was proposed in 1665 by Dr Hooke. According to this extraordinary man, there exists in common air a certain substance which is like, if not the very same with, that which is fixed in saltpetre. This substance has the property of dissolving all combustibles; but only when their temperature is considerably raised. The solution takes place with such rapidity, that it occasions both heat and light; which in his opinion are mere motions. The dissolved substance is partly in the state of air, partly coagulated in a liquid or solid form. The quantity of this solvent present in a given bulk of air is incomparably less than in the same bulk of saltpetre: hence the reason that a combustible continues burning but for a short time in a given bulk of air; the solvent is soon saturated, and then of course the combustion is at an end. Hence also the reason that combustion succeeds best when there is a constant supply of fresh air, and that it may be greatly accelerated by forcing in air with bellows.* Hypothesis of Hooke,

About ten years after the publication of Hooke's *Micrographia*, his theory was adopted by Mayow, without acknowledgment, in a tract which he published at Oxford on saltpetre.† We are indebted to him for a number of very ingenious and important experiments, in which he anticipated several modern chemical philosophers; but his reasoning is for the most part absurd, and the additions which he made to the theory of Hooke are exceedingly extravagant. To the solvent of Hooke he gives the name of *spiritus nitro-aereus*. It consists, he supposes, of very minute particles, which are constantly at variance with the particles of combustibles, and from their quarrels all the changes of things proceed. Fire consists in the rapid motion of these particles, heat in their less rapid motion. The sun is merely nitro-aerial particles and Mayow.

* Hooke's Micrographia, p. 103. See also his Lampas.

† De Sal-nitro et Spiritu Nitro-aereo.

Chap. V. moving with great rapidity. They fill all space. Their motion becomes more languid according to their distance from the sun; and when they approach near the earth, they become pointed, and constitute *cold.**

Hypothesis of Beecher and Stahl. The attention of chemical philosophers was soon drawn away from the theory of Hooke and Mayow to one of a very different kind, first proposed by Beecher, but new-modelled by his disciple Stahl with so much skill, arranged in such an elegant systematic form, and furnished with such numerous, appropriate, and convincing illustrations, that it almost instantly caught the fancy, raised Stahl to the highest rank among philosophers, and constituted him the founder of the Stahlian theory of combustion.

Phlogiston. According to Stahl, all combustible substances contain in them a certain body, known by the name of PHLOGISTON, to which they owe their combustibility. This substance is precisely the same in all combustibles. These bodies of course owe their diversity to other ingredients which they contain, and with which the phlogiston is combined. Combustion, and all its attendant phenomena, depend upon the separation and dissipation of this principle: and when it is once separated, the remainder of the body is incombustible. Phlogiston, according to Stahl, is peculiarly disposed to be affected by a violent whirling motion. The heat and the light, which make their appearance during combustion, are merely two properties of phlogiston when in this state of violent agitation.

The celebrated Macquer was one of the first persons who perceived a striking defect in this theory of Stahl. Sir Isaac Newton had proved that light is a body; it was absurd, there-

* Though Mayow's theory was not original, and though his additions to it be absurd, his tract itself displays great genius, and contains a vast number of new views, which have been fully confirmed by the recent discoveries in chemistry. He pointed out the cause of the increase of weight in metals when calcined; he ascertained the changes produced upon air by respiration and combustion; and employed in his researches an apparatus similar to the present pneumatic apparatus of chemists. Perhaps the most curious part of the whole treatise is his fourteenth chapter, in which he displays a much more accurate knowledge of *affinities*, than any of his contemporaries, or even successors for many years.

fore, to make it a mere property of phlogiston or the element of fire. Macquer accordingly considered phlogiston as nothing else but light fixed in bodies. This opinion was embraced by a great number of the most distinguished chemists; and many ingenious arguments were brought forward to prove its truth. But if phlogiston be only light fixed in bodies, whence comes the heat that manifests itself during combustion? Is this heat merely a property of light? Dr Black proved that heat is capable of combining with, or becoming fixed in bodies which are not combustible, as in ice or water; and concluded of course, that it is not a property but a body. This obliged philosophers to take another view of the nature of phlogiston.

Considered as a subtile matter without gravity.

According to them, there exists a peculiar matter, extremely subtile, capable of penetrating the densest bodies, astonishingly elastic, and the cause of heat, light, magnetism, electricity, and even of gravitation. This matter, the *ether* of Hooke and Newton, is also the substance called phlogiston, which exists in a fixed state in combustible bodies. When set at liberty, it gives to the substances called caloric and light those peculiar motions which produce in us the sensations of heat and light: hence the appearance of caloric and light in every case of combustion; hence, too, the reason that a body after combustion is heavier than it was before; for as phlogiston is itself the cause of gravitation, it would be absurd to suppose that it possesses gravitation. It is more reasonable to consider it as endowed with a principle of levity.

Some time after this last modification of the phlogistic theory, Dr Priestley, who was rapidly extending the boundaries of pneumatic chemistry, repeated many experiments formerly made on combustion by Hooke, Mayow, Boyle, and Hales, besides adding many of his own. He soon found, as they had done before him, that the air in which combustibles had been suffered to burn till they were extinguished, had undergone a very remarkable change; for no combustible would afterwards burn it, and no animal could breathe it without suffocation. He concluded that this change was owing to phlogiston; that the air had combined with that substance; and that air is necessary to combustion, by attracting the phlogiston, for

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which it has a strong affinity. If so, the origin of the heat and light which appear during combustion remains to be accounted for; since phlogiston, if it separates from the combustible merely by combining with air, cannot surely act upon those bodies in whatsoever state we may suppose them.

Heat derived from the air.

The celebrated Dr Crawford was the first person who attempted to solve this difficulty, by applying to the theory of combustion Dr Black's doctrine of latent heat. According to him, the phlogiston of the combustible combines during combustion with the air, and at the same time separates the heat and light with which that fluid had been previously united. The heat and the light, then, which appear during combustion, exist previously in the air. This theory was very different from Stahl's, and certainly a great deal more satisfactory. But still the question, What is phlogiston? remained to be answered.

Phlogiston considered as hydrogen.

Mr Kirwan, who had already raised himself to a high rank among chemical philosophers, by many ingenious investigations of some of the most difficult parts of the science, attempted to answer this question, and to prove that phlogiston is the same with hydrogen. This opinion, which Mr Kirwan informs us was first suggested by the discoveries of Dr Priestley, met with a very favourable reception from the chemical world, and was adopted either in its full extent, or with certain modifications, by Bergman, Morveau, Crell, Wiegleb, Westrumb, Hermbstadt, Karsten, Bewley, Priestley, and Delametherie. The object of Mr Kirwan was to prove, that hydrogen exists as a component part of every combustible body; that during combustion it separates from the combustible body, and combines with the oxygen of the air. This he attempted in a treatise published on purpose, intitled, *An Essay on Phlogiston and the Constitution of Acids.**

* I have omitted, in the historical view given in the text, the hypothesis published in 1777 by Mr Scheele, one of the most extraordinary men that ever existed. When very young, he was bound apprentice to an apothecary at Gottenburgh, where he first felt the impulse of that genius which afterwards made him so conspicuous. He durst not indeed devote himself openly to chemical experiments; but he contrived to make himself master of that science by devoting those hours to study which were

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Combustion explained by Lavoisier.

During these different modifications of the Stahlian theory, he illustrious Lavoisier was assiduously occupied in studying he phenomena of combustion. He seems to have attached imself to this subject, and to have seen the defects of the revailing theory as early as 1770. The first precise notions, owever, of what might be the real nature of combustion, ere suggested to him by Bayen's paper on the oxides of mercury, which he heard read before the Academy of Sciences in 1774. These first notions, or rather conjectures, he pursued with unwearied industry, assisted by the numerous discoveries which were pouring in from all quarters; and by a long series f the most laborious and accurate experiments and disquisiions ever exhibited in chemistry, he fully established the

igned him for sleep. He afterwards went to Sweden, and settled as apothecary at Köping. Here Bergman first found him, saw his merit, d encouraged it, adopted his opinions, defended him with zeal, and took on himself the charge of publishing his treatises. Encouraged and ited by this magnanimous conduct, the genius of Scheele, though assisted by education or wealth, burst forth with astonishing lustre; d at an age when most philosophers are only rising into notice, he had ished a career of discoveries which have no parallel in the annals of emistry. Whoever wishes to behold ingenuity combined with simpliy, whoever wishes to see the inexhaustible resources of chemical alysis; whoever wishes for a model in chemical researches—has only to ruse and to study the works of Scheele.

In 1777, Scheele published a treatise, entitled *Chemical Experiments Air and Fire*, which perhaps exhibits a more striking display of the ent of his genius than all his other publications put together. After a t number of experiments, conducted with astonishing ingenuity, he ncluded, that caloric is composed of a certain quantity of oxygen comed with phlogiston; that radiant heat, a substance which he supposed able of being propagated in straight lines like light, and not capable of mbining with air, is composed of oxygen united with a greater quantity f phlogiston, and light of oxygen united with a still greater quantity. e supposed, too, that the difference between the rays depends upon the antity of phlogiston: the red, according to him, contains the least; the let the most phlogiston. By *phlogiston*, Mr Scheele seems to have ant *hydrogen*. It is needless, therefore, to examine his theory, as it is w known that the combination of hydrogen and oxygen forms not caloric t water. The whole fabric, therefore, has tumbled to the ground; but e importance of the materials will always be admired, and the ruins of e structure must remain eternal monuments of the genius of the builder.

existence of this general law—"In every case of combustion, oxygen combines with the burning body." This noble discovery, the fruit of genius, industry and penetration, has reflected new light on every branch of chemistry, has connected and explained a vast number of facts formerly insulated and inexplicable, and has new-modelled the whole, and moulded it into the form of a science.

After Mr Lavoisier had convinced himself of the existence of this general law, and had published his proofs to the world, it was some time before he was able to gain a single convert, notwithstanding his unwearied assiduity, and the great weight which his talents, his reputation, his fortune, and his situation naturally gave him. At last Mr Berthollet, at a meeting of the Academy of Sciences, in 1785, solemnly renounced his old opinions, and declared himself a convert. Mr Fourcroy, professor of chemistry in Paris, followed his example. And in 1787, Morveau, during a visit to Paris, was prevailed upon to relinquish his former opinions, and embrace those of Lavoisier and his friends. The example of these celebrated men was soon followed by all the young chemists of France.

Mr Lavoisier's explanation of combustion depends upon the two laws discovered by himself and Dr Black. When a combustible body is raised to a certain temperature, it begins to combine with the oxygen of the atmosphere, and this oxygen, during its combination, lets go the caloric and light with which it was combined while in the gaseous state: hence their appearance during every combustion; hence also the change which the combustible undergoes in consequence of combustion.

Thus Lavoisier explained combustion without having recourse to phlogiston; a principle merely supposed to exist, because combustion could not be explained without it. No chemist had been able to exhibit phlogiston in a separate state, or to give any proof of its existence, excepting only its conveniency in explaining combustion. The proof of its existence consisted entirely in the impossibility of explaining combustion without it. Mr Lavoisier, therefore, by giving a satisfactory

planation of combustion, without having recourse to phlo- Chap. V.
ston, proved that there was no reason for supposing any such inciple at all to exist.

But the hypothesis of Mr Kirwan, who made phlogiston same with hydrogen, was not overturned by this explana-t, because there could be no doubt that such a substance as lrogen actually exists. But hydrogen, if it be phlogiston, st constitute a component part of every combustible, and iust separate from the combustible in every case of com-tion. These were points accordingly, which Mr Kirwan ertook to prove. If he failed, or if the very contrary of supposition holds in fact, his hypothesis of course fell to ground.

He refutes the opinion of Kirwan.

Lavoisier and his associates saw at once the important uses ch might be made of Mr Kirwan's essay. By refuting an othesis which had been embraced by the most respectable mists in Europe, their cause would receive an eclat which ild make it irresistible. Accordingly, the essay was trans-d into French, and each of the sections into which it was ided was accompanied by a refutation. Four of the sections e refuted by Lavoisier, three by Berthollet, three by Four-r, two by Morveau, and one by Monge; and, to do the nch chemists justice, never was there a refutation more plete. Mr Kirwan himself, with that candour which dis-uishes superior minds, gave up his opinion as untenable, declared himself a convert to the opinion of Lavoisier.

'hus Mr Lavoisier destroyed the existence of phlogiston gether, and established a theory of combustion almost pre-ly similar to that which had been proposed long before by Hooke. The theory of Hooke is only expressed in general ns; that of Lavoisier is much more particular. The first a hypothesis or fortunate conjecture, which the infant e of the science did not enable him to verify; whereas, voisier was led to his conclusions by accurate experiments, l a train of ingenious and masterly deductions.

According to the theory of Lavoisier, combustion consists two things: first, a decomposition; second, a combination. e oxygen of the atmosphere being in the state of gas, is

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combined with heat and light. During combustion this gas is *decomposed*, its heat and light escape, while its base *combines* with the combustible and forms the product. This product is incombustible; because its base, being already saturated with oxygen, cannot combine with any more.

Lavoisier's theory only applies to common combustion.

13. But the theory of Lavoisier was intended only to explain the combustion of bodies surrounded with atmospherical air or oxygen gas. It does not apply in so satisfactory a manner, when both the supporter and the combustible are in a solid state, and when the product is gaseous. This is the case with gunpowder, which is an intimate mixture of nitre, charcoal, and sulphur. The saltpetre contains abundance of oxygen in a solid state. It is the rapid union of this oxygen with the charcoal and sulphur, and the conversion of them into carbonic acid and sulphurous acid, which occasions the violent combustion and explosive property of gunpowder. Nor does it explain the evolution of fire, when sulphur combines rapidly with copper, or phosphorus with barytes or strontian, or lime. These, and many other similar phenomena, have led modern chemists to the opinion, that combustion is a consequence of the rapid or sudden union of bodies with each other.

Combustion ascribed to electricity.

Sir H. Davy (and his opinion has been embraced by Professor Berzelius) has gone a step farther, and endeavoured to explain the way in which chemical affinity acts with such energy. According to him, all bodies having an affinity for each other are in different states of electricity, the one *plus* and the other *minus*. The more intensely any body is plus and another minus, with the more energy will they unite, and the more violent will be the phenomena of combustion which they will exhibit during their union. Thus oxygen is highly negative, and hydrogen highly positive: hence the energy with which they unite, and the great heat evolved.

Winterl and Oerstedt went a step farther, and endeavoured to account for the heat and the light which appear when bodies unite with great energy. Plus or vitreous electricity, and minus or resinous electricity, in the opinion of the majority of electricians, consist each of a particular *fluid*. These two fluids have a strong affinity for each other; and when they

unite, they constitute (in the opinion of Winterl and Oerstedt) *fire*: hence when a vitreously electric body unites with a resinously electric body, the two fluids combine as well as the bodies in which they were inherent, and make their escape under the form of fire. Chap. VI.

We shall not inquire how far the two parts of this hypothesis are consistent with each other, or how far the doctrine of permanent electric states is consistent with the phenomena of electricity. It will be better to reserve all discussions respecting the electric theory of chemical combination and combustion, till we have taken a view of the principal electric facts which have been ascertained; which will constitute the subject of the second part of this volume.

CHAPTER VI.

OF THE NATURE OF HEAT.

Having made ourselves acquainted with the effects which heat produces on bodies, it may be worth while to make a few observations upon the opinion which this knowledge is calculated to induce us to form respecting the nature of heat. Philosophers are at present divided in their sentiments on this subject. Some consider heat as a peculiar substance, which produces all the different phenomena which we have described, by entering into bodies, combining with them, or leaving them; while others think that it is a property of matter, a motion of a particular kind, the nature of which has never been explained in a satisfactory manner.

Heat considered as a property and as a peculiar substance.

During the 17th century, from the time of Bacon to that of Newton, the latter of these opinions prevailed, at least in this country, almost universally. During the 18th century, the former opinion was equally prevalent; probably from the popularity of Cullen and Black, both of whom taught it in

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their lectures. Since the beginning of the present some of the most popular writers on chemistry in thi have reverted to the old opinion. This was first Count Rumford, who published some curious exp incompatible, in his opinion, with the notion that body. His example was followed by Sir Humph and Dr Thomas Young.

Heat does not alter the weight.

1. From the most careful experiments that it has b sible to make, it may be concluded that no accumu heat in bodies produces any alteration whatever in thei Dr Fordyce, indeed, drew, as a conclusion from an ment of his, that when water is frozen into ice it beco sibly heavier;* but Count Rumford, on repeating t riment, found no sensible difference in the weight o and water. We may therefore conclude, that the increase of weight observed by Dr Fordyce was owin condensation of aqueous vapours upon the surface of vessel containing the frozen water.† Lavoisier had come to the same conclusion from his experiments, wh to have been made with the most scrupulous attenti curacy.—It cannot be shown then that heat, supp peculiar substance, possesses *gravitation*.

First three effects best explained on the supposition that it is a body.

2. The first three of the effects of heat are best e by considering heat as a *substance*; indeed, it is di form any clear conceptions of them upon any other sup If heat be a peculiar substance there is no difficult ceiving how, by its entering into bodies, it increases th and how the bulk diminishes when it is withdrawn. no difficulty in conceiving how more heat may be re produce a given effect upon one body than upon an why the specific heat of different bodies is different supposition that heat is a body; though if heat w motion, neither the property which it has to expan nor the different capacity of bodies for heat, could be e in a satisfactory manner. The radiation of heat adm equally simple explanation, if heat be a body, and so d and evaporation.

* Phil. Trans. 1785, p. 361. † Ibid. 1799, p. 1

Thus these three effects of heat would lead us to consider heat as a peculiar kind of body. Accordingly this is the opinion which all those persons have adopted who have turned their chief attention to expansion, specific heat, radiation, fluidity, and evaporation. Chap. VI.

3. But ignition and combustion do not admit of so easy an explanation, if we admit heat to be a body. We cannot give an intelligible explanation of ignition without admitting that heat and light are mutually convertible into each other. But if this mutual convertibility were admitted, there is nothing in the phenomena of ignition incompatible with the notion that heat is a substance. The last two, if it be a property.

But no satisfactory explanation of the evolution of heat and light, during every case of combustion, has yet been given, owing from the opinion that heat is a peculiar substance. It would be much easier to explain it, if we were permitted to consider heat rather as a property of matter than as a substance *sui generis*. We might then assign a reason why it should make its appearance in all cases of rapid combination, and never in any other case. The explanation that the fire evolved during combustion, is merely a union of the plus and minus electricity, with which the bodies combining together were charged, appears at first sight plausible; but it will not bear a rigid examination; for it is incompatible with the very hypothesis from which it professes to flow. For if chemical affinity be merely the consequence of different states of electricity, and if bodies unite merely because they are in different electrical states, it is clear that they could not continue united unless these different electrical states were permanent. But if the plus and minus electricity were to combine and fly off under the form of fire, there would be an end of the different electrical states which caused the bodies to unite, and, of course, they would cease to continue united, which is contrary to matter of fact.

The phenomena of combustion, then, accord better with the notion that heat is a mere property of matter, while the other effects of heat agree better with the opinion that it is a body. Neither supposition will enable us to explain all the pheno-

Chap. VI. mena. I think, therefore, that it will be safest for us, in the present state of our knowledge, to acknowledge our inability to solve this difficult problem, and to confess that we are incompetent to decide whether it be a substance or a quality.

Whether the phenomena of heat and combustion be incompatible with heat being a body.

4. But Sir Humphry Davy and Count Rumford have made experiments, which they think incompatible with the opinion that heat is a body.

Davy found that a thin metallic plate was heated by friction in the exhausted receiver of an air-pump, even when the apparatus was insulated from bodies capable of supplying heat by being placed on ice. He found also that by rubbing two pieces of ice together so much heat is evolved that the ice melts.

Count Rumford took a cannon cast solid and rough as it came from the foundery; he caused its extremity to be cut off, and formed, in that part, a solid cylinder attached to the cannon $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long. It remained joined to the rest of the metal by a small cylindrical neck. In this cylinder, a hole was bored 3·7 inches in diameter, and 7·2 inches in length. Into this hole was put a blunt steel borer, which by means of horses, was made to rub against its bottom; at the same time a small hole was made in the cylinder perpendicular to the bore, and ending in the solid part a little beyond the end of the bore. This was for introducing a thermometer to measure the heat of the cylinder. The cylinder was wrapt round with flannel to keep in the heat. The borer pressed against the bottom of the hole with a force equal to about 10,000 lbs. avoirdupois, and the cylinder was turned round at the rate of 32 times in a minute. At the beginning of the experiment, the temperature of the cylinder was 60°; at the end of 30 minutes, when it had made 960 revolutions, its temperature was 130°. The quantity of metallic dust or scales produced by this friction amounted to 837 grains.

To make this experiment more striking, Count Rumford contrived to enclose the cylinder above described in a wooden box filled with water, which effectually excluded all air, as the cylinder itself and the borer were surrounded with water, and at the same time did not impede the motion of the instrument.

The quantity of water amounted to 18·77lbs. avoirdupois, and at the beginning of the experiment was at the temperature of 60°. After the cylinder had revolved for an hour at the rate of 32 times in a minute, the temperature of the water was 107°; in 30 minutes more it was 178°; and in two hours and 30 minutes after the experiment began, the water actually boiled. According to the computation of Count Rumford, the heat produced would have been sufficient to heat 26·58lbs. avoirdupois, of ice-cold water boiling hot; and it would have required nine wax candles of a moderate size, burning with a clear flame all the time the experiment lasted, to have produced as much heat. In this experiment all access of water into the hole of the cylinder where the friction took place was prevented. But in another experiment, the result of which was precisely the same, the water was allowed free access.*

The experiments of Rumford were repeated and diversified by M. Haldot. He contrived an apparatus by which two bodies could be pressed against each other by means of a spring, while one of them turned round with the velocity of 2·8 inches per second. The friction took place in a strong box containing 216 cubic inches of water. The results obtained so nearly resemble those of Count Rumford, that it is unnecessary to enter into particular details. The rubber was brass. When the metal rubbed was zinc, the heat evolved was greatest; brass and lead afforded equal heat, but less than zinc; tin produced only $\frac{7}{9}$ths of the heat evolved during the friction of lead. When the pressure was quadrupled, the heat evolved became seven times greater than before. When the rubber was rough, it produced but half as much heat as when smooth. When the apparatus was surrounded by bad conductors of heat, or by non-conductors of electricity, the quantity of heat evolved was diminished.†

In these experiments, it is contended by Davy and Rumford, that no source of this enormous quantity of heat can be pointed out. It cannot be supposed to be absorbed from the neighbouring bodies; nor can it be derived from the bodies

* Nicholson's Jour. ii. 106. † Ibid. xxvi. 30.

Chap. VI. themselves, without supposing the quantity of heat which they contain to be infinitely great.

I am disposed to think that a part at least, if not the whole, of the heat evolved in Count Rumford's experiments, proceeded from an augmentation of density, and a consequent diminution of specific heat in the metallic cylinder subjected to friction. There can be little doubt at least, that the quantity of heat produced depends in some measure upon the size of the cylinder. For surely it will not be contended, that it would be equally great when a very small cylinder is used as when a great one is employed.

Heat from percussion.

Berthollet, Pictet, and Biot, have made a set of experiments, to ascertain the quantity of heat evolved when ductile metals are suddenly struck forcibly, as when they are stamped in the process of coining. The experiments were made upon pieces of gold, silver, and copper, of the same size and shape, and care was taken that all the parts of the apparatus had acquired the same temperature before the experiments began. Copper evolved most heat, silver was next in order, and gold evolved the least. The first blow evolved the most heat, and it diminished gradually, and after the third blow was hardly perceptible. The heat acquired was estimated by throwing the piece of metal struck into a quantity of water, and ascertaining the change of temperature which the water underwent. The following table exhibits the increase of temperature, experienced by two pieces of copper by three successive blows:—

1st Blow	1st Piece	17°·44
	2d Piece	20·80
2d Blow	1st Piece	7·30
	2d Piece	3·69
3d Blow	1st Piece	1·90
	2d Piece	1·46

The whole quantity of heat evolved by each of these pieces of copper is nearly the same; that from the first piece being 26°·64, and that from the second 25°·95.

The following table exhibits the heat evolved from two pieces of silver treated in the same way:—

1st Blow	1st Piece	6°·19
	2d Piece	7·30
2d Blow	1st Piece	5·85
	2d Piece	2·14
3d Blow	1st Piece	2·76
	2d Piece	2·02
Total evolved from the 1st Piece		14·74
Ditto from the 2d		11·46

The change in specific gravity which the metals underwent, was found to be proportional to the heat thus evolved, as appears from the following table, deduced from their experiments: the specific gravities were taken at the temperature 46°·5:—

Specific gravity of gold	19·2357
Ditto annealed	19·2240
Ditto struck	19·2487
Specific gravity of silver	10·4667
Ditto annealed	10·4465
Ditto struck	10·4838
Specific gravity of copper	8·8529
Ditto struck	8·8898
Ditto struck a second time	8·9081

From these experiments it is obvious, that the heat evolved when metals are struck is owing to the condensation, and proportional to the condensation: hence, when they can no longer be condensed, they cease to evolve heat. These philosophers observed, during their experiments, that heat or cold is propagated much more rapidly, from one piece of metal to another, when they are struck, than when they are simply placed in contact.*

There can be little doubt that friction occasions an increase of density as well as hammering. But this increase must be confined to the solid portion, and not exist in the scales or dust rubbed off, which alone Count Rumford examined. Thus

* Mem. d'Arcueil. ii. p. 441.

Chap. VI.

the experiments of Rumford and Davy, though they admit of an easier explanation if we were to consider heat as a mere property of matter, do not furnish the demonstration of the immateriality of heat which these philosophers thought they did.

Mathematical theories of heat.

Two mathematical theories of heat have made their appearance during the last 25 years. The first by Fourier, entitled *Theorie Analytique de la Chaleur*, was partly laid before the French Institute in 1807, and partly the subject of a prize offered by the Institute in 1812. It was published in a separate form in 1822. The second, entitled *Theorie Mathematique de la Chaleur*, was published by Poisson, in 1833. Neither of these eminent mathematicians attempt to settle the question respecting the nature of heat; but investigate the mathematical laws of the radiation of heat, and of its conduction through various bodies. Fourier, from the well known fact that the temperature increases as we descend into the earth, admits the existence of a central heat. From the existence of animal and vegetable remains in the frigid zone, similar to those at present confined to the torrid zone, he infers that the temperature of the earth has sunk considerably, at least in high latitudes. But he demonstrates, contrary to the opinion of Buffon, that its cooling has now reached its maximum. For he shows that the temperature of the surface is entirely regulated by the sun; and that the present effect of a central fire, supposing it to exist, can only be at the utmost, to increase the temperature of the surface by one-thirtieth of a centesimal degree. He shows also that the temperature of the celestial spaces can nowhere be less than 50 or 60 centigrade degrees below zero.

M. Poisson, from a variety of arguments which seem, to me at least, decisive, has rejected the idea of a central heat altogether, and has endeavoured to account for the increasing temperature as we descend towards the centre of the earth, by supposing that the sun with its attendant planets is moving through space, that space contains an alternate warm and cold portion, that while moving through a warm portion of space the sur-

face of the earth becomes hottest, and the temperature sinks as we penetrate towards the centre, that the contrary happens when the solar system is moving through a cold portion of space, which is the case at present. This hypothesis is very bold, and in the actual state of our knowledge is not susceptible of proof. Should a period arrive in the course of ages, when the temperature becomes stationary, or begins to sink as we penetrate into the earth, data will then be obtained for verifying or refuting this hypothesis.

PART II.—OF ELECTRICITY.

Electricity, which occupies so conspicuous a place in modern science, was nearly unknown to the sages of antiquity, and can scarcely be said to have commenced as a science till after the begimning of the 18th century. Part II.

The ancients were aware that *amber* when rubbed acquires the property of attracting light bodies to it.* Theophrastus, speaking of the *lyncurium stone*, observes, that it has an attractive power like that of amber, and that it is said not only to attract straws and small pieces of sticks, but even copper and iron, if they be beaten into thin pieces.† These two facts constitute almost every thing respecting electricity known to the ancients.

Dr Gilbert greatly multiplied the number of substances which are capable of acquiring electrical properties. Mr Boyle made some attempts to explain the phenomena of electricity known in his time, upon mechanical principles.‡ Haukesbee first attended to the light emitted by excited bodies.§ And

* Pliny says, "Cæterum attritu digitorum accepta vi caloris (*succina*) attrahunt in se paleas et folia arida, ut magnes lapis ferrum," Lib. xxxvii. c. 3. But amber seems not to have been distinguished by the ancients from some other transparent resins. Pliny informs us that it grows in India, and that Archelaus, king of Cappadocia, had specimens of it adhering to the bark of trees.

† *Το λυγκουριον ἕλκει γαρ ὥσπερ το ἤλεκτρον· οἱ δε φασιν ου μονον καρφη καὶ ξυλον, ἀλλα και χαλκον καὶ σιδηρον, ἐαν ᾖ λεπτος. ὥσπερ και Διοκλης ἔλεγεν.* Theophrast. *περι των λιθων*, sect. 1. There are very strong reasons for believing that the lyncurium of the ancients is the same mineral with the *tourmalin* of modern mineralogists. See Watson, Phil. Trans. 1759, p. 394.

‡ See Shaw's Boyle, i. 506.

§ Dr Wall first concluded from experiment that all electric bodies became luminous when rubbed. Phil. Trans. 1708, vol. xxvi. p. 69.

Part II. Discoveries of Gray.

his apparatus for exciting electricity probably led the way to the electrical machine.* But it was Mr Stephen Gray who laid the true foundation of electricity, as a science, in the important papers inserted by him in the *Philosophical Transactions* between the years 1720 and 1736. He found that certain bodies can be excited by friction, and others not. This led him to divide bodies into two sets, viz. *electrics* and *non-electrics*. He ascertained that non-electrics may be excited by being brought into the neighbourhood of excited electrics. Finally, he discovered that electricity passes with ease through any length of non-electrical bodies; but not through electrics. This induced him to call the former *conductors*, and the latter *non-conductors* of electricity.

Of Du Fay.

In the year 1733, M. du Fay, of the French Academy of Sciences, began his electrical career. He published in succession eight *Memoirs on Electricity*, equally remarkable for candour and scientific acumen. To him we are indebted for three important discoveries. 1. He found that conductors may be excited by friction, as well as non-conductors, provided we take the precaution to insulate† them before we begin to rub. It was from not attending to this necessary precaution that Mr Gray had failed in exciting these bodies. 2. Electrical bodies, when excited, attract those that are not excited, communicate electricity to them, and repel them. 3. There are two kinds of electricity, the *vitreous* and the *resinous*. The first is produced when we rub smooth glass with a woollen cloth, or with hair; the second when we rub *amber*, *sealing-wax*, or *gum lac*. Bodies having the *same* kind of electricity *repel*, those having *different* electricities *attract* each other. The reason why excited bodies attract light substances to themselves, is, that they produce in them the contrary electricity from that which they themselves possess: hence they are attracted in consequence of the general law that bodies having the opposite

* *Physico-Mechanical Experiments on various subjects. Containing an account of several surprising phenomena touching light and electricity, producible on the attrition of bodies, &c.* 1709.

† By *insulation* is meant surrounding a body with non-conductors, so that no conductors shall be in contact with it.

electricities *attract* each other. The last of M. du Fay's papers on electricity appeared in the *Memoires de l'Academie* for 1737. He died of the small-pox in 1739, at the early age of 41.

Of Cuneus, Muschenbroeck, and Allamand.

In the year 1746 a discovery was made by Mr Cuneus, in company with Messrs Muschenbroeck and Allamand, which attracted the attention of the whole civilized world, and gave such an eclat to electricity, that it for many years drew to itself almost the exclusive attention of men of science. Observing that electrified bodies, when exposed to the common atmosphere, soon lost their electricity, and were capable of retaining but a small quantity, these gentlemen imagined that if an excited body were terminated on all sides by electrics, it might be capable of receiving a stronger power, and retaining it a longer time. A gun-barrel was suspended on silken strings, having one of its ends very near a glass globe, which was turned rapidly, while electricity was excited in it by friction with the hands. A wire was hung from the other extremity of the gun barrel, which dipped into a beer glass full of water, and held in the hands of one of the gentlemen. After the globe had been driven for some time, the gentleman who held the beer glass approached his finger to the gun-barrel to draw a spark from it. The consequence was a *shock*, differing in violence according to the length of time that the globe had been turned. Sometimes it affected only the arms as far as the elbow, sometimes to the shoulders; and sometimes the whole body was affected.* Such was the origin of the Leyden phial experiment, which within a few months was publicly exhibited for money in London and Paris, and all the principal cities of Europe. This famous experiment was immediately repeated in Paris by the Abbé Nollet, and by M. Lemonnier, and in London by Dr Watson. But it was Dr Franklin who first explained it in a satisfactory manner.

Of Dr Franklin.

Dr Franklin first presented himself to the scientific world, as an experimenter on electricity, in 1747. But his *Experiments and Observations on Electricity, made at Philadelphia in*

* Hist. de l'Academie, 1746, p. 1.

Part II. *America*, were not published till the year 1754. This publication forms a remarkable era in the history of electricity. It was translated into almost every European language, and the opinions which it contained, were almost universally adopted. Dr Franklin conceived that only one electric fluid existed,—and that all the phenomena of electricity were occasioned either by its accumulating in bodies, in more than its natural quantity, or by its being withdrawn from bodies so as to leave in them less than the usual quantity. When a body contains its natural quantity of electricity, it exhibits no electrical phenomena whatever. When electricity accumulates in it, the phenomena of the *vitreous* electricity of Du Fay are exhibited. When electricity is deficient, we perceive in it the phenomena of the *resinous* electricity of Du Fay: hence Dr Franklin substituted for *vitreous* and *resinous*, the terms *positive* and *negative*, or *plus* and *minus* electricity.*

Besides this theory, which was probably the cause of the immediate celebrity of Dr Franklin, electricity is indebted to him for three capital discoveries, upon which his reputation as an electrician will finally rest. 1. Electricity is dissipated at a great rate by points, so that it is impossible to accumulate it in pointed bodies. This led to the *thunder rod* as a security for buildings. 2. The second and great discovery of Dr Franklin was that lightning and thunder are occasioned by the accumulation of electricity in the atmosphere; or, in other words, that *lightning is the same thing with electricity*. This he proved by drawing lightning from the heavens, and showing that it possessed all the characters of common electricity. 3. His third discovery was the analysis of the Leyden phial, which contributed more than any thing else, to establish his peculiar theory of electricity.

* I shall employ in the ensuing treatise the terms *positive* and *negative* electricity to denote the two electricities discovered by Du Fay. But I do not adopt the hypothesis of Franklin, that only one electric fluid exists. The phenomena do not seem explicable on any other supposition than the existence of two electric fluids. But the terms *positive* and *negative* applied to these two merely as proper names are more convenient and less objectionable than the terms *vitreous* and *resinous* applied to them by the French electricians.

Part II.

Dr Franklin's theory was exhibited in a mathematical dress by Æpinus in 1759,* and by Mr Cavendish in 1771.† They laid down the hypothesis, and deduced the mathematical consequences from it. These consequences, provided we admit the truth of certain suppositions which Æpinus was obliged to make, flow directly from the theory.

Of Lord Mahon.

In the year 1779, a highly important work on electricity was published by Lord Mahon, afterwards Earl of Stanhope.‡ In this work the author enters at great length into the nature of what are called *Electric Atmospheres.* The term, I believe, was introduced by Dr Franklin, though his notions on the subject want that precision which usually characterises his ideas. Mr Canton demonstrated that the air in the neighbourhood of a charged conductor, or an excited body, gradually acquires the same kind of electricity with which the body itself is charged: so that the air round an excited body, to some unknown distance, is in the same state of electricity with the body itself. The greater the size of the excited body, and the more electricity which it contains, the greater will be the distance to which the surrounding air will be possessed of the same kind of electricity. Now it is this portion of surrounding air which has been called an *electric atmosphere.*

Lord Stanhope demonstrated by very beautiful and decisive experiments, that the quantity of electricity in this electric atmosphere diminishes inversely as the square of the distance from the electrified body: hence the reason why the electrical atmosphere cannot possess any sensible electric properties, at any considerable distance from the electric body. It was this investigation that led Lord Stanhope to the important discovery of the *returning stroke.*

Of Coulomb.

The next series of discoveries on electricity was made by M. Coulomb, and published by him in four Memoirs inserted in the *Memoires de l'Academie de Paris* for the years 1785 and

* *Tentamen theoriæ electricitatis et magnetismi.*

† Phil. Trans. 1771, p. 584.

‡ *Principles of Electricity, containing divers new theorems and experiments, together with an analysis of the superior advantages of high and pointed conductors.*

Part II. 1786. M. Coulomb had constructed a very delicate electrical balance, which measured the attractive and repulsive forces by the tortion of a very fine wire. By means of this balance he made three capital discoveries, which constitute in fact the first principles of all electrical theory. 1. The attractions and repulsions of electrical bodies vary inversely as the square of their distances. So that these forces vary precisely in the same ratio as gravitation does. 2. When insulated bodies are charged with electricity, they do not retain their electricity, but gradually lose it. This is partly owing to the surrounding atmosphere, which never being absolutely free from conducting particles, these particles gradually get possession of the electricity and carry it off. It is partly owing also to the electric bodies, which serve as insulators. There is probably no substance absolutely impervious to electricity; though some are more and some less so: hence the electricity gradually passes off along the insulating body. Coulomb determined the effect of both of these causes, from which the electricity is carried off from an excited body, and thus put it in the power of electricians to calculate how much electricity an insulated body loses in a given time. Such a calculation becomes necessary when we wish to compare the forces of electrical bodies at different distances; for these comparisons being made in succession, could not lead to accurate results unless we ascertain and allow for the electricity lost during the course of the experiment. 3. Coulomb showed that when electricity was accumulated in any body, the whole of it is deposited on the surface, and none of it penetrates into the interior. So that a hollow sphere, however thin its walls be, may be charged with just as much electricity as a solid metallic sphere of the same diameter. It was obvious from this that electricity was not accumulated in bodies, in consequence of any affinity which it has for them; but solely in consequence of its repulsive action.

Electricity, towards the end of the 18th century, had ceased to attract the undivided attention of men of science, as it had done for a considerable, time in consequence of the discovery of the Leyden phial. The rapid advances of chemistry, and

the interesting facts successively brought to light respecting gaseous bodies, had made the study of chemistry the favourite with men of science, and had drawn to it a very great proportion of the most successful cultivators of science in Europe. A new discovery in electricity was necessary to revive the declining popularity of that once favourite science. This discovery was made about the year 1800, by M. Volta, who supplied electricians with a new apparatus, destined not merely to throw a new light upon electricity, but, in the hands of Sir Humphry Davy, to form a new era in chemistry by making us acquainted with the composition of the fixed alkalies and earths.

The discovery of Volta was owing to a set of experiments made by M. Louis Galvani, professor of anatomy in Bologna, about the year 1790. Galvani had dissected a frog, and had laid the hind legs, deprived of their skin, and with the crural nerve laid bare, upon a table, very near the prime conductor of an electrical machine. One of his assistants accidentally brought the point of a scalpel near the crural nerve, while another was drawing sparks from the conductor. He observed that every time that a spark was drawn, while the scalpel was in that position, the muscles of the leg were thrown into violent convulsions. Galvani was informed of this unexpected effect. He immediately repeated the experiment, and satisfied himself that every time that a spark passed from the conductor, while the scalpel approached the crural nerve, a violent convulsion of the muscles of the limb took place.*

Astonished at this effect he made a vast number of experiments, and found that when a piece of metal in contact with the muscles of the leg is made to touch the crural nerve, convulsions ensue; and that these convulsions are more violent if two different metals (silver and copper for example), one in contact with the muscles and the other with the nerve, be made to touch each other. He drew, as an ultimate conclusion, that a peculiar kind of electricity (the nervous fluid of anatomists,)

* Galvani's own account of the discoveries was published in the seventh volume of the *Commentarii de Bononensi Scientiarum et Artium Instituto atque Academia*.

Part II. was secreted in the brain, and conducted by the nerves to the muscles, every fibre of which was charged like a Leyden jar with both the electricities; that the metals served as a conductor, and caused the discharge of this electricity: hence the convulsions.*

The publication of Galvani's paper drew the attention of M. Volta, professor of physics at Pavia. He had already distinguished himself by the discovery of the electrophorus, the condenser, and the eudiometer. He repeated and varied the experiments of Galvani, and drew as a conclusion from them, that the electricity which occasioned the convulsions did not exist in the muscles of the animal, but was evolved by the contact of the two metals. He affirmed that whenever two different conductors are placed in contact, electricity is always excited. This statement occasioned a violent controversy between Volta and Galvani, which continued till the death of the latter in 1799.

Fabroni had also opposed the hypothesis of Galvani in an important paper published in 1792, but which scarcely drew the attention of men of science; though in all probability it constitutes the true explanation of the origin of the electricity which occasioned the convulsions in Galvani's experiments. According to him, it is evolved by the chemical decompositions and new combinations which always take place when two metals are placed in contact with each other.

The dispute between the followers of Galvani and Volta was maintained with undiminished pertinacity by both parties, when Volta, in a letter to Sir Joseph Bankes, in 1800, an-

* There is some reason for believing that these convulsions had been previously observed. Sue, in his *Histoire du Galvanism* (p. 1), states, on the authority of Cotagno, that a mouse having bit the leg of a student of medicine, he caught it and dissected it, and was much surprised on touching the intercostal or phrenic nerve of the animal, to experience a pretty strong electric shock. And Sulzer, in his *Theorie Generale du Plaisir*, says, that if you join a piece of silver and lead so that their edges form one plane, and touch them with the tongue, a strong taste like that of green vitriol is perceived, though each separately give no taste. But these isolated facts drew no attention.

nounced his discovery of the *pile*.* In prosecuting his views, that the electricity which caused the convulsions in the muscles of the frog was evolved from the metals, he found that if a number of pieces of silver and zinc, or copper and zinc, were soldered together two and two, a plate of copper to a plate of zinc; if a number of these pairs were piled above each other, always in the same order (that is, the copper or the zinc always undermost), with a card or piece of leather between each soaked in water, or, still better, in brine, an apparatus was formed, which furnished a constant current of electricity; so that if the wet finger of one hand was applied to the undermost plate, on touching the uppermost plate with the wet finger of the other hand, an electric shock was felt, the violence of which depended on the number of pairs of plates thus piled above each other. These shocks may be repeated as often as the experimenter pleases; so that the pile differs from the Leyden phial in evolving a constant current of electricity, without requiring to be charged. Volta observed that the zinc end of the pile was charged with positive electricity, and the copper end with negative electricity. He found also that when a part of the body deprived of its epidermis was placed in contact with the negative pole, the pain was greater on completing or breaking the circle, than when it was in contact with the positive pole.

Volta commonly made use of an arrangement which he called *couronne des tasses*. It consisted of a number of cups of glass or porcelain, containing each a quantity of salt water. These are all made to communicate in a kind of chain by several metallic arcs, of which one arm or link immersed in one of the cups is copper, and the other immersed in the following cup is zinc; the two being soldered together near the top of the arch. When the two extreme cups are united by plunging the fingers or a wire into them, all the phenomena of the pile are induced.

The first persons who repeated Volta's experiments after the publication of his paper in the *Philosophical Transactions*, were Messrs Nicholson and Carlisle, with a pile consisting of 17 pairs of plates of silver and zinc, separated by moistened

* Phil. Trans. 1800, p. 403.

Part II. card. They observed that the electric current passed throu certain liquid conductors, while it was stopped by bad c ductors. A smell having become sensible in certain cas Nicholson, to discover the cause of it, interposed between t summit and base of the pile, a glass tube filled with wat through the extremities of which passed copper wires comu nicating the one with the top and the other with the bottom the pile. The extremities of the wires in the tube were d tant from each other about two inches. From the wire communication with the silver or negative extremity of t pile, a current of small air bubbles were seen to pass. Fr the wire connected with the zinc or positive end of the pi no air bubbles passed, but the wire became first orange a then black, showing that it had combined with oxygen. T air bubbles from the negative wire consisted of hydrogen g Thus it was proved that the pile has the property of deco posing water, and that the hydrogen is evolved at the wi from the negative pole, while the oxygen is evolved at the wi connected with the positive pole.

Sir Anthony Carlisle repeated the experiment, colouri the water in the tube with litmus. The liquid surroundi the positive wire became red, showing that an acid had be evolved round that wire.

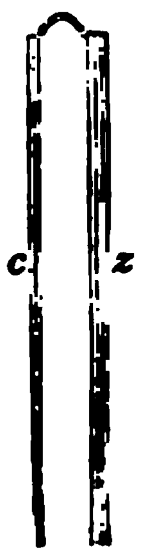
Nicholson observed that the chemical decomposition to place between each pair of the metallic plates; that the su face of the zinc was oxydized; that the common salt wi which the card had been impregnated was decomposed, a an efflorescence of soda deposited on the copper plate. I observed that the effect was increased by augmenting t number of plates: but that increasing or diminishing t thickness of the plates had no effect whatever. When he s stituted platinum wires for copper wires, oxygen gas evolved from the positive wire, and hydrogen gas from negative wire, in the exact proportions to constitute wa showing unequivocally that water was decomposed by electrical energy of the pile. When copper wires were us and the water in the tube acidulated, metallic copper gradually deposited on the negative wire.

These facts, and several others which it is unnecessary to state, were inserted by Mr Nicholson in his quarto journal. Similar experiments were made about the same time by Mr Cruikshanks, of Woolwich, and also by Ritter. It is needless to state the attempts of Cruikshanks to discover the acid and alkali which he conceived to be generated by the action of the pile, because his conjectures did not prove accurate; but he conferred a considerable benefit upon experimenters by substituting a trough of wood, lined with resinous cement and divided into cells, for the original pile of Volta. The plates of copper and zinc were at first soldered together, and inserted into the trough at small intervals from each other, and these intermediate cells being filled with water impregnated with common salt, or still better, acidulated with sulphuric, nitric, or muriatic acid. The two extremities of such a trough being connected by metallic wires, it is in a state of activity.

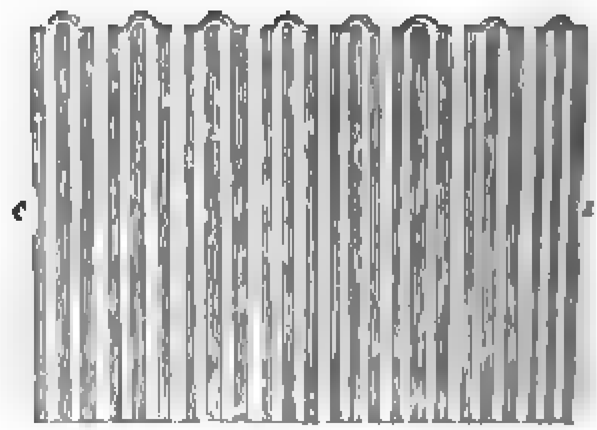
Farther improved.

It was soon observed, that the energy of such a trough did not depend upon the extent of surface in which the copper and zinc plates touched each other; but upon the extent of surface of these plates in contact with the liquid. It was sufficient if the zinc and copper plates touched each other in a single point, provided these plates were themselves plunged into the liquid, so that a copper plate should always be exactly opposite to a zinc plate in the same cell, without the two plates touching each other in any part: hence, instead of soldering the zinc and copper plates together, it was found sufficient to allow a small ribbon of copper to issue from the summit of each copper plate. This ribbon was soldered to the top of the zinc plate. A section of two such plates, is here represented, in which *z* is the zinc plate, and *c* the copper, while 3 represents the ribbon of copper by which the two plates are soldered together. The wooden trough was divided into cells, by cementing into it, at regular distances, as many plates of glass, or varnished wood, as there were plates of zinc and copper cemented together. The trough thus prepared, was filled with the liquid intended to act as a

3



Part II. conductor. The metallic plates were then slipped in, taking care that all the zinc plates were turned the same way, and that each pair was separated from each other by the glass or wooden diaphragm which divided the trough into its various compartments. The figure in the margin represents a section of a small portion of such a trough, in which the middle line in each trio represents the glass plates which divide the trough into different cells, while



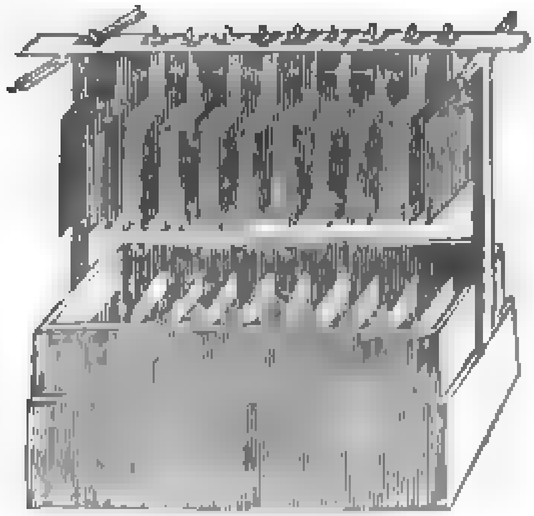
cz, &c., represent eight pairs of the cemented metallic plates slipped over the glass diaphragms, so that there is a zinc plate in every cell, exactly opposite to the corresponding copper plate; each a certain distance from the other. The zinc plates are all turned the same way. At the extremities of the trough, there was a cell filled with the conducting liquid, and containing only one metallic plate. The extremity at which this plate was zinc constituted the positive end of the trough. The other extremity into the last trough of which a copper plate plunged was the negative extremity. There was usually a piece of wood fixed at each extremity of the trough, with a small hole in it for the introduction of a wire, in order to make the two extremities of the trough communicate with each other, which is necessary for bringing the trough into action.

When the size of the metal plates is considerable, four inches square, for example, if the trough contains a considerable number of pairs, 50 or 100 for example, it becomes so heavy as to be almost unmanageable. To obviate this inconvenience, the troughs are now made of porcelain, and of such a size that each trough is divided into ten separate cells, by means of porcelain divisions in the trough. The figure in the margin represents the surface of such a trough, viewed from above. *c*

Porcelain troughs.

c c, &c., represents the different cells into which the trough is divided by the porcelain diaphragms introduced into trough. Part II.

The ten pairs of plates belonging to each trough are properly screwed to a small rectangular piece of wood sufficiently strong to support them, as represented in the margin. The piece of wood to which the plates are screwed is well dried and then varnished, in order to be rendered a non-conductor of electricity. The plates are so



screwed into the wood that a plate of zinc and a plate of copper can be introduced into the same cell at the requisite distance, and exactly opposite to each other. These two plates communicate only by means of the conducting liquid into which they are plunged.

Zinc plates surrounded with copper.

A little reflection is sufficient to show that the energy of each pair of plates depends upon the extent of surface of the two metals exactly opposite to each other, and separated by the conducting surface. When a single plate of zinc is introduced into a trough, and a single plate of copper is placed opposite to it of exactly the same size, it is obvious that only one of the sides of the zinc plate, and one of the sides of the copper plate, exert their energy. The other sides of both plates are totally inefficacious. Now, it will be seen afterwards, that when such troughs are in activity, it is the zinc plate which is wasted and not the copper; but both sides of the zinc plate dissolve. Thus when the trough is constructed in the way now described, the consumption of the zinc is twice as great as its energy. To remedy this defect the copper plate is made to go round the zinc plate, so as to be opposite to it at a certain distance. By this contrivance the two surfaces of the zinc are brought into action. This doubles the energy of the trough without much increasing the consumption of the zinc. This last improvement was suggested by Dr Wollaston.

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Davy seems to have been the first person who attempted to ascertain the effect produced by enlarging the size of the metallic plates. Fourcroy, Vauquelin, and Thenard, had already shown that large plates answered much better than small ones for reddening and burning wires; and for producing those combustions which had been already obtained by means of the common electric battery.* Davy employed a battery composed of 20 pairs of copper and zinc plates, each being a square of 13 inches to the side. When charged with pure water the effects were very trifling. Brine increased the action considerably. With weak nitric acid it was still farther augmented, being capable of heating to whiteness three inches of iron wire $\frac{1}{270}$ of an inch in diameter. He considered the increased effect from nitric acid as owing to the action of that acid on the zinc plates of the battery. When two small pieces of well-burnt charcoal, or a piece of charcoal and a metallic wire, were made to complete the circle in water, vivid sparks were perceived, gas was given out very plentifully, and the points of the charcoal appeared red hot in the fluid for some time after the contact was made, and as long as this appearance existed, elastic fluid was generated with the noise of ebullition. Davy examined the gases produced by means of gold wires and charcoal in water, alcohol, ether, sulphuric acid, and nitric acid, but it is unnecessary to state the results.†

The first important step towards the determination of the chemical decompositions produced by the Voltaic battery was made by Hisinger and Berzelius, and an account of their experiments was published in the year 1803.‡ When they employed iron wires and a dilute solution of sal ammoniac, they obtained hydrogen and oxide of iron. Concentrated ammonia, treated in the same way, gave hydrogen at the negative pole, and azote at the positive pole, while a little oxide of iron was dissolved. When the ammonia was diluted with water, the water alone was decomposed. Sulphate of

* Ann. de Chimie, xxxix. 103. † Nicholson's Jour. iii. 135.
‡ Gehlen's Journal der Chimie, i. 115.

ammonia, with excess of acid, gave hydrogen at the negative pole, and oxygen at the positive. Several alkaline and metallic salts were successively decomposed. Prussiate of ammonia gave at the positive pole prussiate of iron with an excess of oxide. Part II.

Common salt, decomposed by silver wires, gave at the positive pole chlorine and chloride of silver, while the negative pole was surrounded by soda. Sulphate of potash, decomposed by leaden wires, gave peroxide of lead and oxygen at the positive pole, and hydrogen at the negative.

Conclusions of Hisinger and Berzelius.

Hisinger and Berzelius remarked that the evolution of gas continued a long time after the tubes were separated from the electric chain. From these, and many other experiments which it is needless to detail, they drew the following conclusions:—

1. When electricity traverses a liquid, the constituents of that liquid separate so that one set of them collect round the positive pole, and another set round the negative pole.

2. The principles which collect round the same pole have a certain analogy with each other: to the negative side pass combustibles, alkalies, and earths; to the positive pole, oxygen, acids, and oxides.

3. The relative quantity of decomposition in compound liquids is in proportion to the affinity of the components for each other, and to their points of contact with the conductors: hence it may happen that the most dense compound only is decomposed, and that the one most divided is not at all. Thus, concentrated ammonia is easily decomposed; but when it is diluted with water, the aqueous fluid alone undergoes decomposition.

4. The absolute quantity of decomposition is in proportion to the quantity of electricity; and the quantity of electricity is in proportion to the extent of contact of the metals in the battery with the liquid conductors.

5. The worse a conductor of electricity a liquid is, the more difficult it is to decompose it.

6. The phenomena of decomposition are determined by (1.) The affinity of the constituents for the conductor as far

as they can enter into combination with it. (2.) By the reciprocal affinity of the constituents, when there are several. Thus when we decompose nitre, ammonia is formed. (3.) By the cohesion of the new compound.

7. Water is decomposed into hydrogen and oxygen, both of which are insoluble in water. This is the reason why the former is disengaged by the negative wire, and the latter by the positive.

Sulphuric acid gives sulphur at the negative pole, and oxygen at the positive.

The neutral salts deposit their bases at the negative pole, and their acids at the positive.

We shall see afterwards how far these deductions agree with what has been ascertained respecting these decompositions by subsequent experimenters.

Discoveries of Davy.

Such was the state of our knowledge of the chemical action of the Voltaic battery when Davy's celebrated paper on some chemical agencies of electricity appeared in the *Philosophical Transactions* for 1807. Experimenters had been embarrassed by the appearance of an acid and an alkali in their experiments, even when the purest distilled water was used, and various hypothesis had been started to account for these appearances. Davy showed that these appearances were owing to the existence of common salt in the substances employed to contain the water through which the electricity passed. Thus, when he employed an agate tube, muriatic acid and soda were deposited; but, after the current had been allowed to pass for a considerable time, all the common salt in the agate was decomposed, and no more acid or alkali were now deposited.

When animal or vegetable substances were employed to complete the communication, they supplied various salts, by the decomposition of which, alkaline bodies were collected round the negative pole, and acids round the positive. When glass was used to hold the water subjected to decomposition, it was corroded, and supplied an alkali in abundance. When the water was rendered quite pure by repeated distillations at low temperatures, and was put into cups of pure gold, still the appearance of an acid and alkali at the two poles was just

sensible: the acid was the nitric and the alkali ammonia. Davy succeeded in demonstrating that the formation of these bodies was owing to the combination of the azote of the atmosphere with the oxygen and hydrogen set at liberty by the decomposition of water.

Having thus accounted for the appearance of the acids and bases at the two poles of the Voltaic battery, he showed that if the salt to be decomposed was in contact with the negative pole of the battery, the base of the salt was accumulated round that pole, while the acid appeared round the positive pole, even though at a considerable distance, and that it might even be detected in its passage. He employed three glass tubes containing water, and united by filaments of well-washed amianthus moistened with distilled water. The two extreme tubes contained each some common salt dissolved in the water, while in the intermediate tube was placed a solution of sulphate of silver. As soon as the circle was completed, soda began to appear in the negative tube, and chlorine in the positive tube. But the chlorine, in passing through the sulphate of silver in the intermediate tube, produced a heavy precipitate of chloride of silver, while the soda produced a light precipitate of oxide of silver: showing clearly that when the elements of the salt decomposed meet in their passage bodies with which they form insoluble compounds, these compounds are immediately formed, and the elements of the salt are thus removed from the action of the battery.

He showed that the acids and the bases which were thus transported by the electric currents were capable of passing through solutions of bodies for which they had a strong affinity. Solution of sulphate of silver was placed in the negative tube, water in the positive, and ammonia in the intermediate tube, and a current of electricity from 150 pairs of plates of four inches diameter was made to pass through these tubes. The sulphuric acid was collected round the positive pole, and must have therefore traversed the ammonia, for which it had a strong affinity.

From these, and a multitude of other experiments which it is needless to state, Davy drew as a conclusion, that a current

Part II.

of electricity, if sufficiently powerful, is capable of separating two bodies, however strong the affinity may be by which they are united together. This it does by bringing each to the same kind of electricity, either positive or negative; the consequence of which will be that they will repel each other, and of course separate. This led him to infer that bodies having an affinity for each other, are in two opposite electrical states. And he made many experiments to show that when acids and bases are brought into contact, and then separated, the acids are always negative, and the bases positive.

This led him to suggest the well-known theory of chemical affinity, that bodies combine merely because they are in opposite states of electricity, and consequently, that chemical affinity is merely a case of electric attraction. This hypothesis requires us to admit that the electrical state of the atoms of bodies is permanent; and the subsequent investigations of succeeding electricians, especially of Mr Faraday, have given much probability to this opinion, which appeared at first sight inconsistent with the views of electricians, founded chiefly on the Leyden phial, which led to the notion that two bodies in different electrical states could not come in contact without mutually neutralizing each other. Indeed, it seems to follow from Faraday's investigations, that every atom of matter is surrounded with the same quantity of electricity.

Davy examined likewise the two different theories respecting the origin of the energy of the Voltaic battery, which at that time divided electricians:—1st. That of Fabroni, who ascribed it to the chemical decompositions which go on in the battery as long as it possesses any activity. Fabroni's opinion was corroborated by a very ingenious set of experiments of Dr Wollaston.*—2d. That of Volta, who ascribed the whole energy of the battery to the electricity developed by the junction of the pairs of metals; while the liquid interposed between each pair served merely as a vehicle to convey this electricity from one pair to another. Davy adopted the latter of the two views, and supported it by arguments which do not appear to

* Phil. Trans. 1801, p. 427.

me to have much weight: though I am not sure if I fully understand them. At any rate, it would be improper to attempt to discuss the subject here; it will come under our review in a subsequent chapter.

Part II.

The views which Davy detailed in this paper led him to expect to make important chemical discoveries, by subjecting chemical compounds to the action of a powerful Voltaic battery. Nor were his hopes disappointed. In 1808, he decomposed potash and soda, and showed that they were compounds of oxygen and two metals to which he gave the names of potassium and sodium.* He showed also that lime, barytes, strontian, and magnesia, were oxides of metals, to which he gave the names of calcium, barium, strontium, and magnesium.†

Of Thenard and Gay-Lussac.

The success of Davy seems to have stimulated Bonaparte, who had shown a great partiality for Voltaic electricity from the very discovery of the pile by Volta. He offered a prize for the greatest discovery that should be made in Voltaic electricity, to be decided by the French Institute, and to be open for competition to men of science in every nation. The prize was unanimously voted to Davy for his great discovery of the constituents of the fixed alkalies and earths. He assigned a sum of money to be laid out in the construction of a magnificent Galvanic battery, which was intrusted to MM. Thenard and Gay-Lussac, in order to try whether additional discoveries of the same nature with those of Davy might not be made. It consisted of six hundred pairs of copper and zinc plates, each not much less than three feet square. In the year 1811, these philosophers published a work in two volumes, entitled *Recherches Physico-Chimiques*, in which they consigned the result of their experiments with the great battery, and likewise *on the preparation and properties of potassium and sodium, on the decomposition of boracic acid, on fluoric, muriatic, and oxymuriatic acid, on the chemical actions of light*, and *on the analysis of vegetable and animal bodies*. This is one of the most valuable contributions ever made to chemistry, and contained new views and new experiments, which enabled Davy to overturn the received opinions respecting the constitution

* Phil. Trans. 1808, p. 1. † Phil. Trans. 1808, p. 333.

Part II. of muriatic acid and chlorine, and thus to constitu era in chemistry. They constructed both a battery w plates and one with small, investigated with much sa causes which affected their energy, and pointed out of measuring that energy by means of the quantity decomposed in a given time.

Though these experiments led to new and import respecting the mode of action of the battery, they w fortunate as to make any important chemical discove use of either battery. Nor is this to be wondered at, had already, by the munificence of the subscriber Royal Institution, been put in possession of a m battery of 1200 pairs of zinc and copper plates, e inches square, and had found that the loss of electri it was in action was so great from dissipation, that i were very little superior to those of the much smaller already in use. It was impossible to walk across when this immense battery was in action, witho stunned by perpetual shocks.

After the great discoveries of Davy, and of The Gay-Lussac, little, comparatively speaking, remain reaped by succeeding experimenters: hence, after l zeal of electricians began to cool, and, if we exc papers by Davy, and some decompositions, such as silica, by Berzelius, and of alumina, glucina, and Wöhler, and of zirconia, by Berzelius, few contributi to electricity or chemistry were made in the way of sition for ten or eleven years. It may be proper, h notice the dry piles of De Luc and Zamboni, becaus not entirely without their use.

Dry piles. When the common Voltaic pile is used it speedily energy by the liquid in which the cards were soaked dry. In 1803, Hachette and Desormes replaced t by a magma of starch dissolved in water to the thi stiff jelly, in order to try whether, when thus prep pile would not retain its energy longer. In 1809, contrived a pile which apparently was without any li consisted of plates of zinc and paper, gilded on one

laid above each other, the zinc being in contact with the gilded side of the paper. The moisture in the paper was sufficient to charge the pile, and it continued to act till the whole of this moisture was dissipated. In 1812, Zamboni made some improvements in De Luc's column. He piled up, as De Luc had done, several thousand zinc plates and discs of paper, and pressed them strongly together: one side of the paper was covered by tin foil and the other by a very thin crust of black oxide of manganese, rubbed up with a mixture of flour and milk. The humidity of the paper, as in De Luc's pile, was sufficient to maintain the circulation of the electricity, which, in consequence of the badness of the paper as a conductor, is not so energetic as in ordinary piles. It gives sparks to the condensers, but we never can obtain with such a pile either chemical decompositions or the reddening of wires, in consequence of the slowness of the circulation of the electricity in comparison of its motion in the common Voltaic pile. When the humidity of the paper is dissipated, Zamboni's pile, like that of De Luc, loses all its action. I have seen a couple of them keep a pendulum in motion for more than a month.

Discovery of Œrsted.

About the year 1820, the science of electricity, pursued with so much ardour for several years in consequence of the discoveries of Davy, became in a great measure stationary. But in the month of July of that year, Professor John Christian Œrsted, of Copenhagen, guided by theoretical views which he had published twenty years before, announced that the magnetic needle placed at a little distance from a wire joining the two extremities of an active Voltaic battery, was affected in this manner:—If placed above or below the wire, it placed itself at right angles to it, the north pole pointing in one direction when the needle was above, and in the opposite direction when it was below. When placed on the right side of the wire it dipped in one direction, and when placed on the left side it dipped in the opposite direction, so as always to keep itself at right angles to the wire. The direction of the north pole is one way or the other, according to the direction that the positive electricity moves in the wire.*

* Annals of Philosophy, xvi. 273, 375.

Part II.

This important discovery drew the attention of men of science, and was followed by as active and as successful a series of experiments and discoveries, as followed the invention of the Voltaic pile by Volta, or the decomposition of the fixed alkalies by Davy. The first person who took the field was M. Ampere. In a series of memoirs, the first of which appeared very soon after Œrsted's discovery became known to him,* he laid the foundation of *electro-dynamics*, a branch of science including all the phenomena of electricity in motion. In this memoir he reduced the phenomena observed by Œrsted to two principal facts. He showed that the electro-dynamical force exists equally in all parts of the conducting wire, as well as in the pile. To explain why the needle points different ways, according as it is placed above the pile or by the side of the conducting wire, he indicated this general law, which determines the position of the needle in every particular case:—Suppose the electricity to move from the positive pole of the pile to the negative pole: suppose a man to make part of the circuit, so that the current enters by his feet and passes out by his head, the needle will point towards his left hand.

Views of Ampere.

In this valuable memoir he pointed out the importance of rolling the conducting wire into helices or spirals, in order to increase the effect. This led afterwards to the formation of galvanometers, consisting of magnetic needles poised freely, and surrounded by wires, coated with silk thread to prevent contact, and twisted into spirals.

In the same paper he showed that two wires through which electrical currents were moving, *attracted* each other if the currents were moving in the same direction, but *repelled* each other if the currents were moving in opposite directions. He gives his ideas respecting the property of magnets, which he refers to electrical currents circulating round molecules on planes perpendicular to their axes. He conceives that in the terrestrial globe there are electrical currents in planes, perpendicular to that of the dipping needle, and consequently moving from east to west.

* Ann. de Chim. et de Phys. xv. 59, 170.

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Soon after, he announced to the Academy of Sciences that the action of the terrestrial globe has a tendency to bring the plane or moveable portion of a conductor of an active Voltaic pile into a position perpendicular to the dipping needle.

In 1822 he reduced the attractions and repulsions of electrical currents to calculation.* His object was to show that all the facts relative to the mutual action of two magnets, a Voltaic conductor and a magnet, or two conductors, might be referred to a single cause, consisting in a force sometimes attractive, sometimes repulsive, between infinitely small portions of what he called electric currents, always acting in the direction of the line which joins their middle. He admits that the action of this force is not a mere function of the distance; but depends also on the angles which determine the respective position of two infinitely small portions of the electric currents of the line which joins their centres. Ampere concluded from his experiments, and observations, that electricity and magnetism were identical; or, in other words, that magnets owed their peculiar properties to electric currents passing through them.

A spiral wire communicates magnetism.

M. Arago had observed that the metallic conductor of an active Voltaic pile attracts iron filings, and possesses all the properties of a magnet, whatever be the metal of which it is composed.† He observed that if a copper wire be wound spirally round a glass tube, and then made to unite the two poles of a galvanic pile, if a steel needle be introduced into the inside of the tube, it is instantly converted into a magnet. If the needle be left too long in the tube, the magnetic properties are again weakened or even destroyed. The best way is to hold the needle in the hand, and introduce it about half way into the tube, and then withdraw it: the magnetic virtue will be acquired as completely as if it had been wholly within the tube. If common electricity be made to pass along the spiral conducting wire, the needle is equally converted into a magnet.

M. De la Rive fixed two small plates of zinc and copper beside each other, and attached them to a piece of cork. The

* Ann. de Chim. et de Phys. xx. 398.

† Ibid. xv. 93.

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two plates were connected together by a brass wire wound into a spiral. When this little apparatus was placed on the surface of an acid liquid, the plate being undermost and allowed to float at pleasure, it arranged itself into the magnetic meridian in consequence of the polarity of the spiral wire.

Discoveries of Faraday.

Next to the discoveries of Professor Œrsted, those of Mr Faraday on electro-magnetism* are by far the most important. He first observed that the position of the magnetic needle, with respect to the conducting wire, greatly modified the effects produced. He ascertained that the apparent attraction of the needle on one side, and its consequent repulsion on the other, did not occur under all circumstances; but that according as the wire was placed nearer to or farther from the pivot of the needle, attractions or repulsions were produced on the same side of the wire. When the wire is made to approach perpendicularly towards one pole of the needle, the pole will pass off on one side in that direction which the attraction and repulsion at the extreme point of the pole would give; but if the wire be continually made to approach the centre of motion by either the one or the other side of the needle, the tendency to move in the other direction diminishes. It then becomes null, and the needle is quite indifferent to the wire; and ultimately the motion is reversed, and the needle powerfully endeavours to pass the opposite way.

Rotation of the wire round the magnet.

From these facts, Mr Faraday concluded that the centre of magnetic action or the true pole of the needle is not placed at its extremity, but in its axis at a little distance from the extremity and towards the middle,—that this point has a tendency to revolve round the wire, and necessarily therefore the wire round the point. And as the same effects in the opposite direction take place with the other pole, Mr Faraday concluded that each pole had the power of acting on the wire by itself, and not as any part of the needle or as connected with the opposite pole. The attractions and repulsions, he viewed merely as exhibitions of the revolving motion in different parts of the circle.

* They will be found in the 12th volume of the Journal of the Royal Institution.

A

After many unsuccessful attempts, Mr Faraday at last succeeded in verifying the existence of this revolving motion by the following ingenious apparatus. A small quantity of mercury was put into the bottom of the tube A, (see fig. in the margin,) closed below with a cork, and a small magnet, *b*, was passed through the cork, and made to project above the surface of the mercury. A piece of clean copper wire, *c*, was taken about two inches in length, and amalgamated at both ends. A small loop is forme at one end, by hich it was suspended from a wire, passing through a cork in the upper extremity of the tube, and terminating in another loop. This gave the copper wire free motion. It was of such a length, that the moveable end just dipped into the mercury. The mercury was now connected with one of the poles of a galvanic battery by means of the magnet, while the wire at the top of the tube was connected with the other pole. The moveable copper wire immediately began to revolve round the magnetic pole, and continued to do so as long as the contact continued. On bringing the magnetic pole from the centre of motion to the side of the wire, there was neither attraction nor repulsion; but the wire endeavoured to pass off in a circle, still leaving the pole for its centre, and that either on one side or the other, according to circumstances. Part II.

All the directions of the motion are reducible to two. When a current of electricity passes through the wire, the north pole rotates in one direction, and the south pole in the other. Suppose the extremity of the moveable wire dipping into the mercury to be negative, then the motion is in the same direction as that of the hands of a watch, if we suppose the watch lying with its face corresponding with the surface of the mercury in the tube. If the lower point of the moveable wire be positive, the revolution takes place in the contrary direction. If the wire be made fast, and the magnetic pole be made to revolve round, the motion is similar and in the same direction.

This first simple arrangement was varied exceedingly by Mr Faraday and other persons, and a great variety of pieces

Part II. of apparatus contrived to exhibit the revolutions of magnetic poles round conducting wires, or rather indeed of conducting wires round magnetic poles.* Many curious and amusing exhibitions have been the result, though very little has been added to Mr Faraday's original and important discovery.

We are indebted to Mr Snow Harris† for a set of experiments on the magnetic energy of different metals, by which they are made to rotate by the action of a rotatory horse-shoe magnet. The following table exhibits this energy :—

Rolled silver.	Rolled copper.	Cast copper.	Rolled gold.	Cast zinc.	Cast tin.	Cast lead.	Solid mercury.	Cast antimony.	Fluid mercury.	Cast bismuth.
39	29	20	16	10	6·9	3·7	2	1·3	1	0·45

Mr Barlow has endeavoured to show that the earth receives its magnetism from electricity by induction.‡

Mr Fox has shown that the intensity of terrestrial magnetism varies slightly according to the different seasons of the year.§

Mr Faraday, in 1831, ascertained that electric currents are produced by magnets.‖

I ought to have mentioned, that Biot and Savart demonstrated, as early as 1820, the law according to which the attraction or repulsion of two electrical currents diminishes as the distance increases. They showed that it decreases exactly in proportion to the distance.¶

Mr Faraday had tried whether a magnet might not be made to rotate round its axis, but could not succeed. Ampere showed that the reason was, that in Faraday's method of operating the current neither passed in the magnet nor in the portion of the conductor that was solidly attached to it. By rectifying the apparatus he caused the magnet to rotate with great velocity.**

* These, it would not suit the limited views of the author of this very short sketch, to describe here. A considerable number of them will be seen figured and described in Mr Watkins' *Popular Sketch of Electro-magnetism and Electro-dynamics*, published in 1828. Taylor, London.

† Phil. Trans. 1831, p. 85. ‡ Ibid. p. 99. § Ibid. p. 199.

‖ Ibid. 1832. p. 163. ¶ Ann. de Chim. et de Phys. xv. 232.

** Ibid. xxii. 389.

Part II.

Davy made an important experiment to show that the action of magnets on conductors, or of conductors upon other conductors, was entirely owing to the attractions and repulsions of the electric current—the matter of which the magnets or conductors were composed, not contributing any thing to the effect.* When the great battery of the London Institution, consisting of 2000 double plates of zinc and copper, was charged, and the poles connected by charcoal, an arc of electrical light, varying from 1 to 4 inches, was obtained, of such intensity that the eye could not support the glare. When a magnet was presented to this arc of light, it underwent the same action as a moveable metallic conductor would have done; that is to say, it was attracted or repelled according to the pole presented, regard being had to the direction of the electric current.

Rotation by a magnet.

About the year 1824, M. Arago discovered that when plates of copper and other substances are set in rapid rotation beneath a magnetized needle, they cause it to deviate from its direction, and finally drag it along with them. This curious fact was repeated and verified by Messrs Herschell and Babbage in the year 1825. These gentlemen then mounted a powerful compound horse-shoe magnet capable of lifting 20 pounds, in such a manner, as to receive a rapid rotation about its axis of symmetry placed vertically, the line joining the poles being horizontal and the poles upwards. A circular disc of copper, 6 inches in diameter, and 0·85 inch thick, was suspended centrally over it by a silk thread without torsion, just capable of supporting it. A sheet of paper properly stretched was interposed, and no sooner was the magnet set in rotation than the copper commenced revolving in the same direction, at first slowly, but with a velocity gradually and steadily accelerating. The motion of the magnet being reversed, the velocity of the copper was gradually destroyed; it rested for an instant, and then immediately commenced revolving in the opposite direction. This alteration of direction might be repeated at pleasure.

* Phil. Trans. 1821, p. 425.

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The rate of rotation was not diminished by interposi tween the copper and the revolving magnet plates of glass, wood, copper, tin, zinc, lead, bismuth, or ant But it was greatly diminished when the substance inte was iron. One sheet of tin plate reduced the rate o tion to about $\frac{1}{5}$th part, and two sheets almost destr altogether.

When discs of other metals were substituted for c they were found also capable of being made to rotate, with different degrees of rapidity. The order of the di metals, and the intensity of the force of each, that of being reckoned 1, was as follows:—

	Force.
Copper . . .	1
Zinc	0·90
Tin	0·47
Lead	0·25
Antimony . . .	0·11
Bismuth . . .	0·01
Wood . . .	0·00

When these discs were cut with a pair of scissors in radii, stretching from the circumference nearly to the the force was greatly weakened; but it was nearly r again by soldering these intervals with any metal whate

Silver was found to hold a high rank among the meta gold a very low one. Mercury lies between antimo bismuth. The only other substance in which any tr magnetism has been perceived besides the metals, is cl in that curious state in which it is deposited at a red hea coal gas, lining the retorts and rendering them gr useless.

Copper revolving produced no effect upon copper. always necessary, in order that motion should be pro that one of the bodies should be a magnet. It is obviou this that magnetism is introduced into copper, and the metals simply by induction. Messrs Babbage and He have explained in a very ingenious manner, how in conse

of this magnetism by induction, the rotation produced in these experiments may be explained.*

These experiments have been repeated, and some interesting new facts discovered concerning such rotations by Mr Snow Harris.†

I consider it unnecessary to continue this historical sketch any farther. The numerous and important facts respecting electro-magnetism which have been ascertained within these few years, could hardly be understood without entering into details which will come in better in a subsequent chapter of this work. But it would be unpardonable not to mention the important labours of Faraday, which have thrown so much light upon almost every department of electricity, which have corrected so many mistakes, and determined so many first principles. These have been consigned in fourteen memoirs, published successively in the *Philosophical Transactions* between the years 1832 and 1839, both inclusive.‡

CHAPTER I.

GENERAL PRINCIPLES.

There are some properties of bodies, so essential (so to speak) to their existence, that we can scarcely conceive them to be wanting without destroying the body altogether. Thus gold is *extended* and possessed of *weight*. Nor can we conceive this metal to exist at all if we were to suppose it to be deprived of *extension*. There are other properties not so essential to the existence of bodies. They may be wanting, or they may

* Phil. Trans. 1825, p. 467. See also valuable experiments on the same subject by Mr Christie, Ibid. p. 497.

† Phil. Trans. 1831, p. 497.

‡ Ibid. 1832, pp. 125, 163; 1833, pp. 23, 507, 675; 1834, pp. 55, 77, 425; 1835, pp. 41, 263; 1838, pp. 1, 79, 83, 125.

Part II. be present, without altering the nature of the body in which they occasionally reside. Lead is not always fluid; but it may be rendered fluid by heating it sufficiently. Iron does not always *point to the north*, but it may be made to do so by communicating magnetism to it, and then suspending it, so that it is at liberty to turn freely in every direction. Glass has not always the property of drawing light bodies to it, and then repelling them again. But if we rub it with the dry and warm hand, or still better, if we rub it with a piece of leather covered over with an amalgam of zinc and tin, it acquires this property.

Electrical properties, what.

1. Thus glass, and many other bodies, acquire by friction a property which they did not possess before—the property of alternately *attracting* and *repelling* light bodies. Now this is the property which is distinguished by the name of *electricity*. It was first observed in *amber*, to which the Greeks gave the name of ἤλεκτρος: hence the term *electricity*.

2. If we make the excited tube approach the cheek, we have a sensation resembling the application of a cobweb to the cheek.

The spark.

3. If we make our finger approach within a small distance of the rubbed tube, we hear a snap, and a spark passes between the tube and the finger. In the dark, the glass tube appears covered with a bluish light.

When glass or any other substance has thus acquired the property of attracting light bodies, it is said to be *excited*.

4. What is the cause of the new properties induced in these bodies by friction? We are altogether ignorant of it; though a variety of hypothetical explanations have been advanced. Some have ascribed the new properties to the agency of a peculiar fluid, existing in all bodies, and called the *electrical fluid;* but not sensible till it either accumulates beyond the usual quantity, or till it becomes actually deficient. Others suppose the existence of two distinct fluids, which they endow with certain properties to enable them to explain the electrical phenomena. While a third set deny the existence of an electric fluid altogether, and consider the attractions and repulsions as mere properties induced into matter. Whatever the cause

of these new properties may be, we may distinguish it, although unknown, by applying to it the term *electricity*. Chap. I.

5. Glass, sulphur, sealing-wax, resins, gums, silk, wool, hair, and even paper and wood, if well dried, may be excited by friction; but when we attempt to excite a piece of metal by the same process, we find it impossible to succeed. It was supposed at first, that metals were incapable of being excited by friction, and on this account all substances were divided into two classes, namely, *electrics* and *non-electrics*—the first consisting of those bodies which may be excited by friction, the second of those that cannot. But it was afterwards ascertained, that metals do not want the power of being excited by friction; but they are unable to retain the electricity after they have obtained it. If we place a plate of metal upon a cake of sealing-wax or gum lac, and rub it with the fur of a cat, we shall find upon examining it in the usual way, that it has become excited.

Electrics and non-electrics.

Excitation.

Bodies thus supported on *electrics*, are said to be *insulated*. Insulation must always be attended to when we wish to induce any electricity in metallic bodies. Coulomb has shown that the best insulator is *gum lac*. Glass in order to insulate should be varnished; if not, it does not insulate except in dry weather. A silk thread in order to insulate should be of some length.

Insulation.

Even the friction of fluids upon solid bodies is sufficient to excite them. Mercury shaken in the inside of a glass tube electrifies the tube. If a wooden vessel containing mercury be cemented on the top of a glass receiver, and the receiver be exhausted by means of an air-pump, the mercury is driven by the atmosphere through the pores of the wood, and falls in a shower into the receiver, striking against its sides. The jar by this shower of mercury is excited. In like manner, we can excite glass by blowing on it with a pair of bellows. Whether two gases produce electricity when they rub against each other, as is the case when two opposite currents of air cross each other, has not been determined. But analogy renders the thing very probable. In this way probably is induced part of the electricity which almost always exists in the atmosphere, and which occasions so many important meteorological phenomena.

6. An excited body attracts and repels light bodies at a

Part II. distance—nor is the effect prevented by interposing a solid body. Thus, if we suspend within a glass jar, a small ball composed of the pith of elder, about the tenth part of an inch in diameter, by a fine silk thread, it will be attracted to an excited glass tube placed on the outside of the jar.

Conductors and non-conductors.

7. The reason why some bodies are easily excited by friction, while others cannot, depends upon the facility with which electricity is capable of moving through them. Through the former it moves very slowly or not at all—but through the latter it moves with great facility. The former are *non-conductors* or *bad conductors* of electricity; the latter are *good conductors*.

Metals and charcoal, water and all liquids (oils excepted), are good conductors. Melted wax and tallow are good conductors; but these bodies, while solid, conduct very badly. The flame of alcohol and ice are good conductors.

Glass, resins, gums, sealing-wax, silk, sulphur, precious stones, oxides, air, and all gases, are non-conductors, or at least very bad conductors.

If a conductor, how long soever it may be, be brought into contact (by one extremity) with an excited non-conductor, it instantly takes a portion of the electricity from the non-conductor. Indeed, if the conductor be in communication with the earth, the whole accumulated electricity will soon pass along the conductor, and be dissipated in the earth: so that we shall not be able to find any traces of electricity either in the non-conductor or conductor. But if the conductor be insulated, by suspending it from silk strings, for example, or supporting it by cylinders of gum lac, it will become excited as well as the non-conductor; and the electricity will pass along the whole of it, so that the extremity farthest removed from the excited electric will exhibit the phenomena of electricity to as great an extent as the extremity nearest the excited electric. Suppose an iron or copper rod twenty or thirty feet in length to be suspended on silk strings, and that an excited glass tube be placed in contact with one extremity of it, the other extremity will immediately exhibit all the usual phenomena of an excited body. But if we substitute a rod of glass or sealing-wax in place of the metallic rod, it will not exhibit

any sensible electricity at the remote end, even although its length should not exceed a few inches.

If we suspend a small pith ball by a slender metallic wire, and touch it with an excited glass tube, we shall find that it will not retain the communicated electricity an instant; but if the pith ball be suspended by a silk thread, it will retain the electricity communicated to it by an excited glass tube for a considerable time, if the day be dry, and the air clear, when the trial is made.

Dry air is a non-conductor, but moist air is a conductor: hence the reason that electrical experiments do not succeed in rainy or foggy weather. We may, however, in general succeed in producing electrical phenomena whatever be the state of the atmosphere, if we take care to dry all the substances which we employ, and keep them dry by bringing them into the neighbourhood of a good fire; or by covering them with a thin coating of tallow, which keeps the moisture at such a distance that we may charge a Leyden jar, as usual, even in a very moist atmosphere. If we use the tallow, we must beware of such a heat as would melt it, for, becoming a conductor by fusion, all electric action would be destroyed. This fire has the effect also, by raising the temperature of the air, of increasing its dryness, and thus preventing it from carrying off the electricity so rapidly as it would otherwise do.

Electricity does not alter the bulk of bodies.

8. Electricity does not alter the bulk of bodies. The bulk of a piece of metal is not increased by charging it with electricity, nor does the bulk of a glass tube, or stick of sealing-wax, alter when it is excited, except so far as the friction may increase the temperature of the body.

Neither is the nature of the electricity altered by the temperature of the body which contains it. An electric spark drawn from *ice* does not feel *cold*, nor does a spark feel hot when drawn from red hot iron. Dufay was of opinion, that a spark drawn from a living body was more painful, and communicated a much more burning sensation, than when drawn from dead matter.

Its motion very rapid.

9. Electricity moves through conducting bodies with such rapidity, that no interval of time whatever can be perceived

Part II.

during the transit, how long soever it may be. Dr Watson, and several other members of the Royal Society, made an electric charge pass through a wire, extending more than two miles, or 12,276 feet. One gentleman held a charged Leyden jar in one hand, and the extremity of the wire in the other. Another gentleman held the other extremity of the wire in one hand, and with the other hand brought a short iron rod in contact with the knob of the Leyden phial. The consequence was a discharge of electricity, and both the gentlemen received the shock at the same instant of time. So that electricity passes over a circuit of two miles instantaneously, or at least in an interval of time too short to be noticed.* Indeed in one of Dr Watson's experiments the circuit through which the electricity passed was nearly six miles in length, yet no sensible interval of time was taken up by it in this long extent through which it had to pass.

Mr Wheatstone repeated Dr Watson's experiments in a different and more refined manner, and though he did not succeed in determining the velocity, he concluded from his experiments that electricity moves through conducting bodies with a velocity greater than that of light, and consequently not slower than 200,000 miles in a second.†

Bodies do not retain electricity in a vacuum.

10. It has been found that bodies cannot be made to retain electricity in the vacuum of an air-pump. The electricity flies off as fast as it is evolved, so that no substance can become charged: hence it is obvious that the presence of dry air is necessary to prevent the electricity of excited bodies from being dissipated. It is commonly supposed that air produces this effect by the pressure which it exerts upon the surface of the excited body. But it is much more natural to ascribe it to the state of electricity which the surrounding atmosphere acquires. It has been demonstrated by the experiments of Mr Canton, and Lord Stanhope, that the air surrounding an excited body acquires the same kind of electricity with the body itself. The intensity of this electricity of the air diminishes inversely, as the square of the distance: hence

* Phil. Trans. 1747, p. 49; 1748, p. 491.

† Ibid. 1834, p. 583.

at a small distance from the excited body it becomes insensible. Now, as electricities of the same kind *repel* each other, it is obvious that the electricity of the air surrounding an excited body will repel the electricity of the excited body; and this repulsion being the same on all sides, it must have the effect of preventing the electricity of the excited body from being dissipated. When this repulsive force is withdrawn there is nothing to oppose the repulsive force which the particles of electricity exert against each other: hence they must be immediately dissipated.

Two kinds of electricity.

11. If we suspend two small pith balls, by means of very slender wires, from a stick of sealing-wax, and rub the wax with a dry woollen cloth, the two balls will *repel* each other. If we suspend another pith ball, by a slender wire from a stick of sealing-wax, and rub it also with a dry woollen cloth, this pith ball, if brought near the two former, will also be *repelled* by them. If we suspend the pith balls by slender wires from dry glass tubes, and rub the tubes with dry woollen cloth, the result will be the same—all the pith balls will repel each other. But if we suspend a small pith ball, by a slender wire from a glass tube, and another in a similar way from a stick of sealing-wax, and make the two balls approach, after having rubbed the glass tube and the stick of sealing-wax, the pith balls, instead of repelling, will *attract* each other.

Hence it is obvious that the electricity excited in glass is different from that excited in sealing-wax: since bodies having the electricity of glass, *attract* those having the electricity of sealing-wax; while bodies having each, either the electricity of glass, or of sealing-wax, *repel* each other. M. Dufay, the discoverer of this most important fact, distinguished these two electricities by the names of *vitreous* and *resinous*, from the two substances in which he first perceived them. Dr Franklin afterwards termed them *positive* and *negative* electricities, and these terms have come into general use.

Positive and negative.

Bodies charged with the same kind of electricity, whether positive or negative, *repel* each other; but bodies charged with different electricities *attract* each other. Bodies charged with positive electricity, *repel* bodies charged with positive

Part II. electricity, and *attract* bodies charged with negative electricity. Bodies charged with negative electricity, *repel* bodies charged with negative electricity, and *attract* bodies charged with positive electricity.

Friction evolves both electricities. 12. When two substances capable of being excited, are rubbed against each other, both acquire electricity, but each is charged with a different kind. One becomes positively electrified, and the other negatively. This law holds universally; but what kind of electricity each substance acquires depends upon the substance against which it is rubbed. For example, if we rub dry woollen cloth against *smooth glass*, it acquires *negative* electricity, while the glass becomes *positively* electrified. But if we rub woollen cloth against *rough glass*, it acquires *positive* electricity, while the glass is charged with *negative* electricity. The following table contains a number of electric substances arranged in such a way, that when they are rubbed against each other, the substance first in the list becomes positively, and the substance below it negatively electrified:—

1 Fur of a cat,	6 Paper,
2 Smooth glass,	7 Silk,
3 Woollen cloth,	8 Lac,
4 Feathers,	9 Rough glass,
5 Wood,	10 Sulphur.

The fur of a cat acquires positive electricity against what substance soever it be rubbed. Sulphur acquires negative electricity against whatever substance it be rubbed. Feathers acquire negative electricity when rubbed against the fur of a cat, smooth glass, or woollen cloth; but they acquire positive electricity when rubbed against wood, paper, silk, lac, rough glass, and sulphur. The same observation applies to every substance in the table. It assumes *negative* electricity when rubbed against any substance *above* it in the table, and *positive* electricity when rubbed against any substance *below* in.

Electricity by induction. 13. When an insulated conductor is brought into the neighbourhood of an insulated charged conductor, its electricity undergoes a new arrangement. The end of it next the excited conductor assumes a state of electricity opposite to

that of the excited conductor; while the farther extremity assumes the same kind of electricity. Suppose a conductor charged with positive electricity: the end of the insulated conductor next it becomes negative, and the end farthest off positive; and intermediate between these two points, there occurs a place where neither positive nor negative electricity can be perceived. This place is called the *neutral point.*

This is the reason why light bodies are attracted by excited electrics. They acquire the opposite electricity, and of course are attracted. When they touch the excited electric, they acquire the same kind of electricity which it has, and consequently are repelled. This electricity they speedily lose by coming in contact with conductors connected with the earth. They are again attracted, again repelled; and these alternate attractions and repulsions continue till the electricity of the excited body is dissipated.

Electricity evolved by fusion, heat, pressure, &c.

14. Though friction is the usual way of producing electricity, yet it is evolved in many other ways. It has been ascertained that when bodies are *melted* and allowed to congeal again, they frequently become electric. It has been found that some bodies become electric by *pressure*. There are several mineral bodies, the most remarkable among which is the *tourmalin*, that acquire marked electric properties when their *temperature is elevated* a certain number of degrees; which properties they lose again when they are allowed to cool. Few chemical *combinations* or *decompositions* take place without evolving electricity. Finally, Volta has shown that when two metals (supposing both insulated) are placed in contact and then separated, both give signs of electricity; the one being always positively, and the other negatively electrified.

But the most important of all the methods of evolving electricity hitherto discovered, is that by which it is produced in the Voltaic pile, a full account of which will be given in a subsequent chapter. At first it was considered that Voltaic electricity differed in its nature from electricity evolved by friction; but Dr Wollaston, in a set of experiments contrived with much sagacity, showed that both electricities were

Part II. similar, being capable of producing the same effects. He proved also that the evolution of both electricities was owing to chemical action.* The subject was taken up by Mr Faraday in the third series of his *Researches on Electricity*, and he showed, by a copious induction, the identity of electricity derived from different sources; namely, Voltaic electricity, common electricity, thermo-electricity, and animal electricity, and pointed out at the same time in what the apparent differences between them consists.† Mr Faraday showed likewise that the same absolute quantity of electricity produces the same effect upon a galvanometer,‡ whatever be its intensity. He showed also, that the chemical decompositions, (the decomposition of water for instance,) like the magnetic force, are in direct proportion to the absolute quantity of electricity that passes, without regard to the tension.§ And these results have been confirmed by the experiments of Pouillet.‖

CHAPTER II.

OF ELECTRICAL APPARATUS.

Electrical investigations being conducted by means of the electrical machine, it will be requisite before proceeding any farther with an account of the phenomena of their investigation, to give a short description of the electrical machines at present used by electricians.

There are two distinct kinds of electrical machines at present employed; namely, the *cylindrical*, and the *plate* machine.

The cylindrical machine undoubtedly originated from the

* Phil. Trans. 1801, p. 427. † Ibid, 1833, p. 23.

‡ An instrument by which the electricity is indicated and measured by the deflection of a magnetic needle. It will be described in a subsequent chapter.

§ Phil. Trans. 1833, p. 48.

‖ See Becquerel's Traité de l'Electricité et du Magnetism, v. 275

glass globes of Mr Hauksbee. He fitted them upon an axis, which he turned round rapidly by means of a winch, and subjected them to the friction of the hand, or of a woollen cloth. After the discovery of the Leyden phial, improvements in this simple apparatus were successively introduced. Oiled silk was found to answer better as a rubber than the hand. Cushions of leather stuffed with hair or wool, and made as smooth and equal as possible, were found still preferable; and Mr Canton discovered, in 1762, that if a quantity of amalgam of tin, mixed with some whiting, be spread upon the cushion, it would excite the globe to a great degree with very little friction.*

It was gradually found that glass cylinders might be substituted for the globes with advantage; and these cylinders at last came to be generally substituted for the globes. The cylindrical machine at present used in this country, was the contrivance of Mr Edward Nairne, a very ingenious philosophical instrument-maker in London. His first account of it appeared in the *Philosophical Transactions* for 1774; but he afterwards added considerably to its simplicity and its elegance.

Cylindrical machine.

Nairne's cylindrical machine consists essentially of a glass cylinder of any dimensions, supported on two glass pillars upon a mahogany board. This glass cylinder has at one extremity a winch, with a handle by means of which it may be turned briskly round. On each side of the glass cylinder and parallel to it, are two cylinders of tin plate japanned, or of polished brass, nearly as long as the glass cylinder, but not so thick, and terminated at each end by a hemispherical surface, so as to avoid all angles or points, and to make the whole cylinders as smooth and equable as possible. These cylinders are supported each by a glass pillar of the same size and height as the pillars which support the glass cylinder. These two cylinders are called the two prime conductors of the machine. To one of them is fixed the cushion, which is to act as a rubber. This cushion consists of leather stuffed with hair,

* Phil. Trans. vol. lii. p. 461

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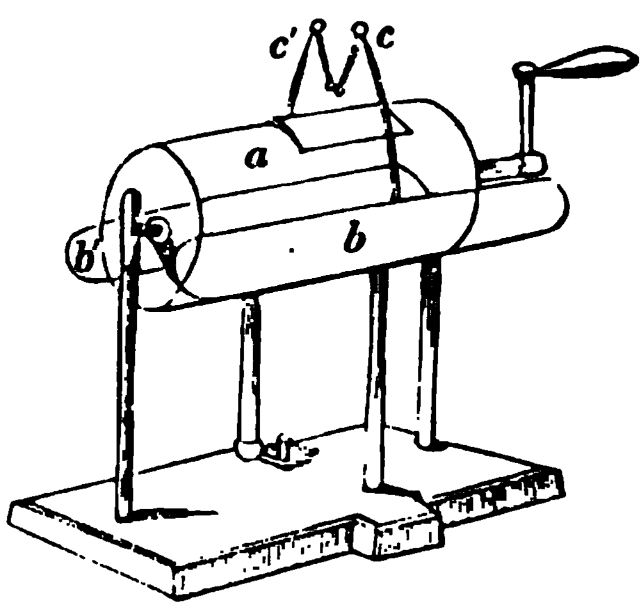
and made by means of a spring to press equably against the glass cylinder. To the cushion is attached likewise a flap of black silk, which covers the upper part of the glass cylinder, and prevents the electricity from being dissipated till it reach the conductor on the opposite side.* This second conductor is precisely similar to the first; only instead of a cushion it has a number of pointed brass wires attached to its inside, pointing directly to the glass cylinder, and approaching as near it as possible, so as not to touch it. The use of these pointed wires is to draw the electricity from the glass cylinder, in proportion as it is evolved, and to conduct it to the conductor to which they are attached. Each of these conductors has a certain number of holes in its surface, to which wires, electrometers, &c. may be attached, and into which brass knobs may be inserted to regulate the discharge of the electricity.



The figure in the margin represents a view of Nairne's cylindrical machine.

In it *a* is the glass cylinder, and *b b'* the two prime conductors. *c, c'* are two electrometers attached to the two conductors, to show that they are in different electrical states, the one to which the rubber is attached being negative, and the other positive: hence the two electrometers are attracted when the machine is worked.

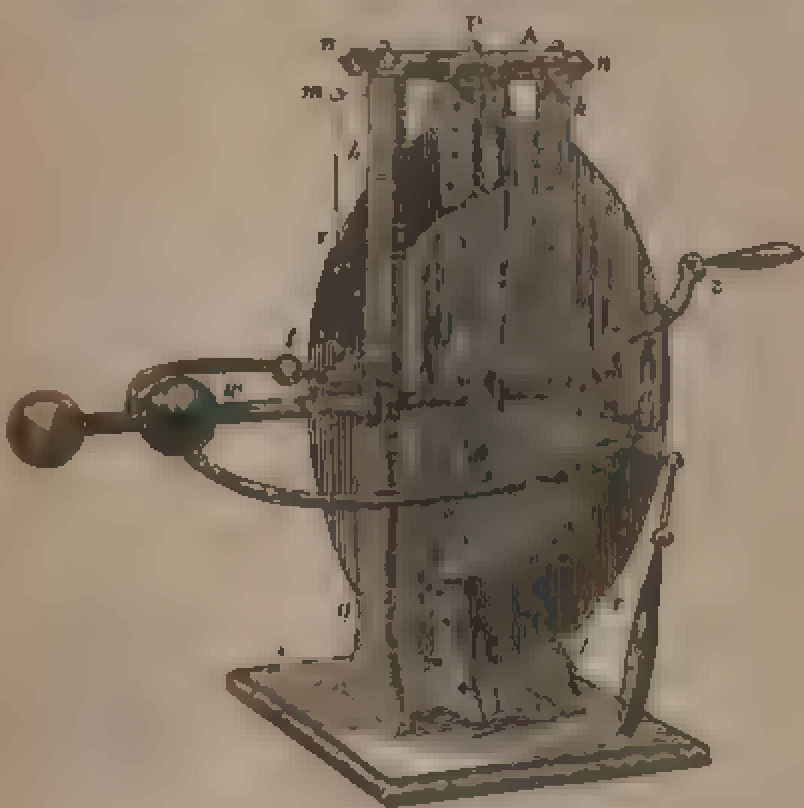
The prime conductor to which the cushion is attached becomes charged with negative electricity when the machine is put in motion; while the opposite prime conductor becomes charged with positive electricity. That the machine may be capable of acting, it is requisite that the prime conductor to which the cushion is attached, should be connected with the ground by means of a metallic chain suspended from it.

* This flap is greatly improved when it becomes stained with grease: hence an old flap will be always found to act better than a new one. The new one becomes immediately as good as the old, if we impregnate it with tallow.

Chap. II.

Plate machine.

Plate machines were first brought to a state of perfection by Mr Cuthbertson, when he settled in Amsterdam about the



year 1769. The above figure represents his plate machine, as it is now in common use. A C D is the mahogany stand to which all the different parts of the machine are fixed. C D is a square piece of mahogany which forms the basis of the machine, about two feet long, one broad, and an inch and a half thick. To this bottom two upright beams or stiles are fixed, which constitute the wooden frame of the machine. *g h*, one of these beams, is a straight wooden stile, glued fast to C. *k l* is the other beam or stile, not glued as the former, but fixed by three screws—one at the front to draw it close to C D, and two under the bottom, to draw the stile downwards upon the bottom, that it may be secure against any motion. *n n* is a cross piece of wood which forms the top of the frame, screwed fast to the two stiles by two brass screws, which pass through the cross piece into *h k*, by which means it can be drawn so tight as to secure it against any motion.

This plate machine has four rubbers, two fixed at the top, and two at the bottom of the frame. *o* is the spring frame at the top, which contains the upper pair of rubbers, and is

Part II. screwed to A by a screw, P. *q* is the spring frame which contains the under pair of rubbers. *r*, *r*, is prepared silk, sewed to each rubber, and seen separate in the next figure.

a, *t*, *w*, is the prime conductor. *w*, *x*, is a solid stick of glass, which serves to support, and also to insulate the conductor. The end *w* is mounted with brass, which screws into the centre of the large ball of the conductor. *y*, *y*, is a round plate of mirror glass, fixed upon an axis, one end of which turns in a hole in the stile *g*, *h*, and the other runs through a hole in the middle of the stile *k*, *l*, and is turned by means of a winch, *z*, which causes the plate to revolve and pass between the rubbers fixed at the top and bottom of the frame

Fig. 1.

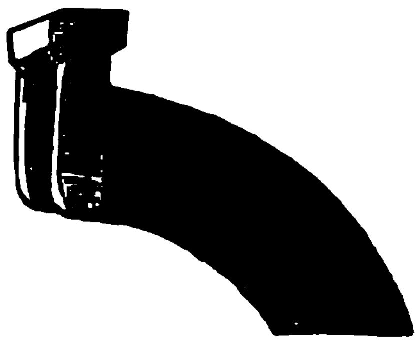


Fig. 1, represents the spring frame of the upper cushions on one side, and the silk, for the sake of distinction, is taken off.

Fig. 2.

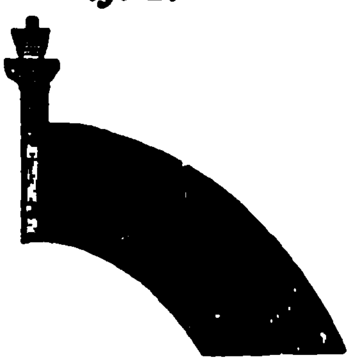


Fig. 2, represents a single rubber with the silk flap. It is covered with leather, and stuffed with slips of woollen cloth. Then a piece of silk, properly prepared, and cut to the shape, is sewed fast to the middle of the rubber. By turning the screw one way it will draw the rubbers close, and by turning it the other it will loosen them. As it is difficult to get at the head of this screw to turn it with the fingers, there are holes made at the side of the head of the screw in which the end of a wire may be put, by which it may be easily turned. To fix the screw frame and rubbers to the machine, open the rubbers to about the width of half an inch, then slip them upon the plate so that the top comes close under *n*, *n*. Then put the long screw, P, through the hole in the top of the machine, and screw it into the top of the frame of the rubbers, very tight, taking particular care that the plane of the rubbers applies to the plane of the plate. This will hold the rubbers in their proper place. *m*, is a wire from which proceeds two or three silk cords fastened to the edge of the silk flaps, which serve to prevent them adhering to the edge of the plate while turning.

The rubbers, *q*, are constructed exactly as those already described, but are fixed differently to the frame of the machine. Chap. II.

The piece, *a*, *b*, which forms the bottom of the frame

for the under rubber, is cut open from one side quite to the centre of the piece, and at the bottom of the frame of the machine is a piece of brass placed upright with a male screw, to which is fitted a nut with a female screw, having holes at the edge for the convenience of screwing it tight by means of a wire. The two sides with the rubbers, are placed upon the bottom of the machine, so that the upright brass pin goes into the groove, and is there screwed tight by the female nut.

a, *t*, *w*, is the prime conductor of the machine. *w*, is a large brass ball with a shank about an inch long fitted into the cylindric part, so that it may be turned for the purpose of placing the hole, which is seen at one side of the ball, in any position that the experiment may require. This hole is useful for a variety of purposes. The conductor is supported and insulated by the solid glass cylinder, W, *x*. If required at any time to be taken to pieces, the cylinder part of the conductor must be unscrewed from the large ball, where there is a square nut, which being unscrewed, W, *x*, may be taken out. One end of the two receiving arms or tubes, *t*, *a*, is screwed into the large ball by W, one having a left-handed screw so that the arm may be supported by the shoulder to prevent it falling by its weight. The two cross pieces fronting the plate, with two or three points to receive the excited fluid, are each screwed on to their respective arms, one with a right-handed screw, the other with a left-handed one, for the same purpose as the screws at the other end of the arms. The end, *x*, of the glass cylinder is mounted with a joint, which fits into the frame of the machine opposite the hole in which the axis moves.

To give this machine all its energy, the rubbers require to be covered with amalgam. This is made by taking equal weights of zinc and tin, and melting them in a crucible. The melted mixture is poured into twice its weight of mercury in a wooden box made for the purpose. The box is to be well shaken till the metals are cold. The amalgam is then to be Amalgam.

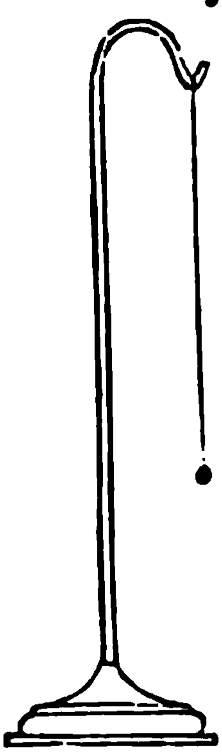
Part II. reduced in a metal mortar to a very fine powder, and afterwards mixed with a sufficient quantity of hog's lard to make it into a paste. I believe the best proportions for making amalgam would be.

Zinc . . .	8·25 parts
Tin . . .	7·25
Mercury . .	37·5

Such an amalgam, when kept, is apt to crystallize. But it is rendered fit for use by pounding it in a mortar before using it.

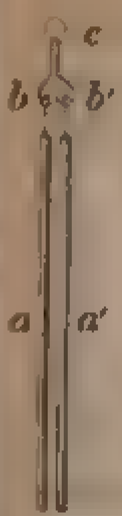
When the machine is to be used, the rubbers should be taken off and cleaned by scraping off the old amalgam with a knife and rubbing the silk with a cloth. The rubbers should then be placed in a warm place till they are well dried. New amalgam must now be spread on with a knife. It should be laid on flat and smooth, and to such a thickness that the surface of the amalgam is level with the surface of the silk, and there should be no openings or separations between the amalgam and the silk.

Electrometers. A variety of instruments have been invented for detecting electricity when it exists. These instruments have received



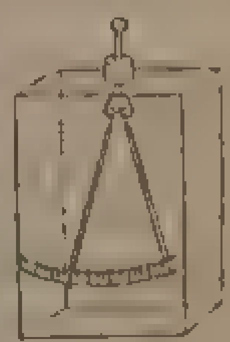
the name of *electrometers*. The simplest of these is the pendulum electrometer represented in the margin. It consists of a glass road fixed in a sole, and bent at the top so as to form a hook. From this hook hangs a thread of raw silk, to the bottom of which is fixed a very small ball made of the pith of the elder, quite round and weighing only a small fraction of a grain. When an excited glass rod or any similar body is brought near this pith ball it is immediately attracted, and the stronger the electricity the farther will the ball be drawn from the perpendicular.

Another common and very useful electrometer is represented in the margin. It consists of two pieces of straw, a, a', both smooth and of the same size and length, suspended parallel to each other, and at a very small distance



from each other by two very fine metallic wires bent at the top into hooks, which are attached to two small holes, b, b', in a small piece of metal terminating above in a knob, c. When any excited body is made to touch the knob, c, the straws, which are very light and hang freely suspended, acquiring the electricity of the touching body immediately separate from each other, and indicate the existence of electricity. And the farther they separate it is obvious that the quantity of electricity must be the greater.

To prevent the agitation of the air and the other accidents which would speedily derange or destroy the straws, the electrometer is usually enclosed in a square glass box as here represented, upon one of the faces of which is pasted a circular division to mark the degree to which the straws separate. The neck and upper part of this glass box is varnished with gum lac, in order that it may insulate the better.

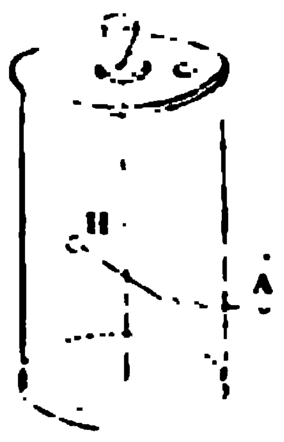


Instead of straws, slips of gold leaf are employed with advantage. This constitutes what is called Bennet's gold leaf electrometer, which is a very delicate instrument, and capable of indicating very minute quantities of electricity. See a description, *Phil. Trans.* 1787, p. 26.

In these and many other common electrometers which I think it needless to describe, the instrument cannot be considered as a true measurer of the quantity of electricity, because, as the two straws or the two slips of gold leaf separate farther and farther from each other, it is evident that gravitation will act more and more powerfully to bring them back again to their naturally vertical position: hence the repulsive force of the straws or leaf is not proportional to the distance to which they separate from each other. These instruments cannot of course be employed to measure the energy of electricity.

But the electrometer of Coulomb is free from this defect. It is represented in the margin. It consists of a glass vessel having a lid also of glass, in the centre of which a small hole

Part II.



is drilled. Through this hole passes an untwisted raw silk thread four inches long, and fixed at the top to a micrometer, by means of which it may be turned round any number of degrees at pleasure. To the silk thread is attached a very fine gum lac thread, H, having at each extremity a small knob. This lac needle with its knobs weighs only ¼ grain. A small hole is drilled in the side of the vessel, at A, through which passes a fine wire terminated at both extremities by a knob. When an excited body is placed in contact with the knob at A, the knob at the other extremity will acquire the same electricity as the excited body. This electricity it will communicate to the knob of the lac needle suspended by the silk thread which was previously almost in contact, and the two knobs will repel each other. The moveable knob attached by the silk thread will separate from the other, and the quantity of electricity will be proportional to the distance to which it is driven off.

Coulomb's balance.

Coulomb's electrical balance is an instrument intended to measure the quantity of electricity in bodies, and indispensable in accurate experiments. *Fig.* 1, (p. 337,) represents this instrument as originally constructed by Coulomb.

A, B, C, D, is a glass cylinder 12 inches in diameter, and 12 inches in height. On the top of it is placed a glass plate 13 inches in diameter, which entirely covers the glass cylinder. This plate is pierced with two holes about 20 lines in diameter. One of these is in the middle at *f*, on which is elevated a glass tube 24 inches in height. This tube is cemented over the hole, *f*, by the common cement used for electrical machines. At its upper end, at *h*, is placed a micrometer of torsion seen in detail in figure 2. The upper part (fig. 2,) carries the button *b*, the index *i*, *o*, and the pincers of suspension, *q*. This piece enters into the hole G, (fig. 2.) The piece in which the hole G is situated, is formed of a circle, divided into 360 degrees, and of a copper tube Φ, which enters into the tube H, soldered to the inside of the upper extremity of the glass tube, *f*, *h*, (fig. 1). The pincers, *q*, (fig. 2,) have very nearly the form of the point of a black lead pencil, and the

Fig. 1.

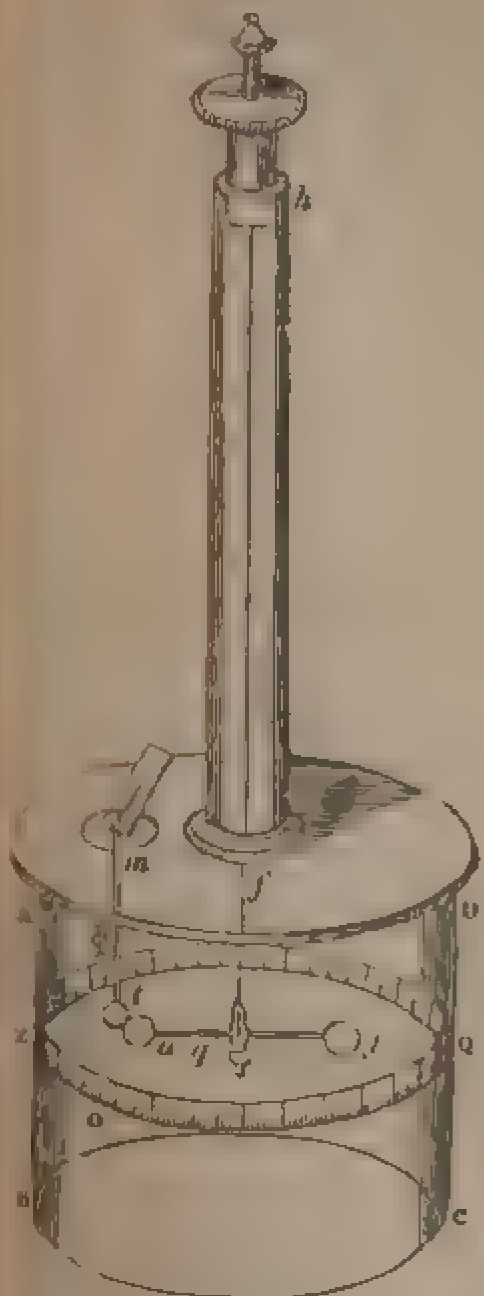


Fig. 2.

Fig. 3.

two sides may be forced together by means of the ring *q*. These pincers support the extremity of a very fine silver wire. The other extremity of this wire is laid hold of at P (fig. 3), by the pincers of the cylinder, P, O, of copper or iron; the diameter of which does not exceed a line, and the extremity of which, P, is slit, forming a pair of pincers which are shut by the ring, Φ. The little cylinder swells out, and is pierced at C to pass through it the needle, *a*, *g*, (figures 1 and 3). The weight of this little cylinder must be sufficient to stretch the silver wire without breaking it. The needle, *a*, *g*, seen suspended horizontally in figure 1, about the middle of the large

s cylinder in which it is contained, is formed either of a thread covered with sealing-wax, or of a straw equally vered with sealing-wax, and terminated from q to a, a space 18 lines, by a fine cylinder of gum lac. At the extremity, of this needle, is a little ball of elder pith from two to three ines in diameter. At g is a vertical plane of paper imbibed with turpentine, which serves to counterbalance the ball, a, and to retard its oscillations.

The cover, A, D, is pierced with another hole at m. By this second hole is introduced a small cylinder, m, Φ, t, the lower part of which, Φ, t, is of gum lac. At t is a ball equally of the pith of the elder. Round the glass vessel, at the height of the needle, the circle, Z, Q, is described, and divided into 360 degrees; or a slip of paper divided into 360 degrees may be pasted round the glass at the height of the needle.

To use this instrument the cover is so placed that the hole, m, corresponds with the 0° of the circle, Z, O, Q. Then the index of the micrometer, o, i, is fixed at the degree, 0°, or the first division of the micrometer. The whole micrometer is then turned in the tube, f, h, till, looking by the vertical wire which suspends the needle and the centre of the ball, the needle, a, g, is found to correspond with the first division of the circle, Z, O, Q. Then, by the hole, m, the other ball, t, suspended by the thread, m, Φ, t, is introduced, so that it touches the ball, a, and that looking by the centre of the thread of suspension, and the ball, t, we observe the first division, O, of the circle, Z, O, Q. The balance is now fit for all the different operations which it is calculated to perform.

This very ingenious and important instrument is liable t certain mechanical defects, which sometimes render the use it rather difficult. One of the most troublesome of these the swinging of the needle, generally set in motion whenc we turn the micrometer, or charge the insulated balls. remedy these defects Mr Snow Harris has contrived an genious modification of the torsion balance, a descripti which, together with many experiments showing its u will be found in the *Philosophical Transactions* for p. 417.

Many other instruments, chiefly connected with Voltaic and magneto-electricity, have been contrived and are in constant use. But an account of them will be better understood after we have described the nature and properties of the Voltaic pile, than it would be at present. Chap. III.

CHAPTER III.

OF THE RATE AT WHICH CHARGED BODIES LOSE THEIR ELECTRICITY.

For our knowledge of this subject, which constitutes an essential preliminary to all other electrical investigations, we are indebted to the sagacity of Coulomb.

Bodies lose electricity.

It is well known that an electrified conducting body, though insulated, soon loses its electricity. This is occasioned by two causes. 1. The air always contains conducting bodies, (the particles of vapour for example,) which gradually carry off the electricity by coming in contact with the electrified body, imbibing its electricity, and then flying off. The rapidity of the action of these conducting bodies will vary with their number, which is exceedingly different according to the different states of the atmosphere. 2. No substance seems to be absolutely impervious to electricity; all seem to allow it to pass, though with greater or less rapidity: hence the electricity passes slowly off along the substance employed as an insulator. The rapidity with which it escapes in this direction, will depend upon the nature of the insulating body. Glass has such a tendency to condense vapour that it is a very bad insulator, except in a very dry atmosphere. It answers better if it be covered over with a varnish, which puts an end to the tendency of vapour to condense on its surface. Resinous bodies answer better as insulators than any other, because moisture has but little tendency to be deposited on them from the atmosphere:

Part II. hence this great source of the loss of electricity does not exist in them. Coulomb found, that when the charge was not great, a small cylinder of *sealing-wax* or of *lac*, about $\frac{1}{27}$th of an inch in diameter, and from $\frac{3}{4}$ths to $\frac{7}{8}$ths of an inch in length, insulates completely a sphere of elder pith about half an inch in diameter. He found also that when the air was very dry, a very fine silk thread passed through boiling sealing-wax insulates perfectly, if it be 5 or 6 inches long, and the pith ball be not highly charged with electricity.*

Effect of the air, in carrying off electricity.

1. Coulomb's first set of experiments were made to determine the rate at which a body loses its electricity by the contact of atmospherical air. He suspended to a very fine silk thread covered with sealing-wax, and terminated by a small cylinder of gum lac 18 or 20 lines long, a small sphericle of elder pith similar to that on the needle of his electric balance. He introduced it by the hole in the lid of the balance, and brought it in contact with the pith ball on the needle. An insulated pin with a large head, was charged with electricity and brought in contact with one of the balls, by which an equal quantity of electricity was imparted to both. The two balls mutually repelled each other and separated to such a distance, that the force of torsion of the wire of the balance was equal to the repulsive force of the balls. Let us suppose the ball of the needle driven to the distance of 40°. By twisting the suspending wire, he brought it to a smaller distance, 20° for example. Let us suppose that this was produced by twisting the thread of suspension 140°. The moment of time when the ball corresponds exactly to 20° is marked. As the electricity is dissipated, the balls in a few minutes will begin to approach each other. To preserve them always at the same distance of 20°, the suspending thread is untwisted, by means of the index, 30 degrees, and in consequence of this untwisting, the balls separate from each other a little more then 20 degrees. The instant that the balls approach each other within 20° is marked, and the interval of time which has elapsed between the two observations, is accurately noted down. Suppose this

* Mem. Paris, 1785, p. 612.

interval to be 3 minutes. It is clear that the repulsive force at the time of the first observation was measured by 140 + 20 = 160 degrees. Three minutes after it was measured by 110 + 20 = 130 degrees; that is to say, it had diminished 30°, or at the rate of 10 degrees per minute. As the mean force between the two observations is measured by 145 degrees, and as it diminishes 30° in 3′, or 10° per minute; the electric force of the two balls diminishes $\frac{10}{145}$ per minute. It was in this way that Coulomb formed the following table, representing observations made on the 28th of May, the 29th of May, the 22d June, and 2d July. These four days were chosen because on them the heat was nearly the same, but there was a considerable difference in the degree of moisture existing in the atmosphere on each, as indicated by Saussure's hygrometer :—

	Time of experiment.	Distance of the balls.	Torsion of the micrometer.	Time elapsed between two consecutive observations.	Electric force lost between two observations.	Mean force between two observations.	Ratio of electric force lost per min. to the mean force of the body.
1st Experiment, 28th May, Saussure's Hygrometer 75°. Thermometer 66°·875. Barometer 29·979 inches.							
1st Exper.	6h 32′ 30″	30°	120°	5[illegible]	20	140	[illegible]
2d ...	6 38 15	30	100	6[illegible]	20	120	[illegible]
3d ...	6 44 30	30	80	8[illegible]	20	100	[illegible]
4th ...	6 53 0	30	60	10	20	80	[illegible]
5th ...	7 3 0	30	40	14	20	60	[illegible]
6th ...	7 17 0	30	20				
2d Experiment, 29th May, Hygrometer 69°. Thermometer 66°·875. Barometer 30·008 inches.							
1st Exper.	5h 45′ 30″	30	130	7[illegible]	20	150	[illegible]
2d ...	5 53 0	30	110	9[illegible]	20	130	[illegible]
3d ...	6 2 30	30	90	9[illegible]	20	110	[illegible]
4th ...	6 12 15	30	70	20[illegible]	30	75	[illegible]
5th ...	6 33 30	30	40	18	20	60	[illegible]
6th ...	6 51 0	30	20				
3d Experiment, 22d June, Hygrometer 87°. Thermometer 67°·437. Barometer 29·75 inches.							
1st Exper.	11h 53′ 45″	20	80	3	20	90	[illegible]
2d ...	11 56 45	20	60	3	20	70	[illegible]
3d ...	11 59 45	20	40	5¼	20	50	[illegible]
4th ...	12 5 0	20	20	11¼	25	28	[illegible]
5th ...	12 16 15	20	5				
4th Experiment, 2d July, Hygrometer 80°. Thermometer 67°·437. Barometer 29·929 inches.							
1st Exper.	7h 43′ 40″	20	80	5[illegible]	20	90	[illegible]
2d ...	7 49 0	20	60	8[illegible]	20	70	[illegible]
3d ...	7 57 20	20	40	12	20	50	[illegible]
4th ...	8 9 15	20	20	8[illegible]	10	35	[illegible]
5th ...	8 17 30	20	10				

From the last column of the preceding table, it is obvious, that when the experiments are made on the same day, and in the same circumstances, the rate of electricity lost to the total force is a constant quantity; and that this rate varies in proportion as the hygrometer indicates a variation in the humidity of the air: hence it follows, that for the same state of the air the loss of electricity is always proportional to the electric density.

It is obvious, that the ratios in the seventh column of the preceding table, refer to the loss of electricity sustained per minute by the two balls. And as the two balls were equal, the loss of electricity sustained per minute by one of the balls, will be only half the quantity given in this column. The mean loss of electricity sustained by the two balls on the 28th May, being $\frac{1}{40 \cdot 3}$, it is clear that the loss sustained by one of the balls will only be $\frac{1}{80}$.

Depends upon moisture.

We see that the loss of electricity from the contact of air, depends upon the moisture contained in it. For it is greatest when the hygrometer is highest, and least when the hygrometer is lowest. A more extensive set of experiments is still wanting to enable us to determine whether the loss be all owing to the moisture present; and if it be, at what rate it increases with the increase of moisture.

Coulomb determined experimentally, that the loss of electricity per minute is the same, whatever the nature of the electrified body may be, and whatever its shape, if it be not highly electrified. For in that case, bodies which have sharp edges or angles lose their electricity much faster than bodies which have no such angles or edges.

Loss of electricity from imperfect insulation.

2. To determine the loss of electricity from imperfect insulation, the natural method would appear to be to employ insulating substances, which possess the property of insulating very imperfectly; but when this is done, the electricity goes off so fast, that there is not time for measuring by means of the balance the rate at which the charge diminishes. This induced Coulomb to employ a pretty good insulator. The pith ball attached to the needle was perfectly insulated as in the first set of experiments; but the ball introduced through

Part II. the hole in the balance, instead of being insulated, as in the former experiments, by a small cylinder of gum lac 15 or 18 lines in length, was supported by a thread of raw silk, such as it comes from the cocoon, and fifteen inches long.

The following table exhibits the results of two sets of experiments made precisely as in the former table, and on the same days, as the dates will show, namely, the 28th and 29th of May. The former table shows the loss of electricity on these days from the contact of air: hence, by comparing the first table with the second, we can easily determine the quantity of electricity lost in a given time along the insulating substances employed as a support:—

	Time of experiment.	Distance of balls.	Torsion of micrometer.	Time between two successive observations.	Electric force lost between two observations.	Mean force between two observations.	Rate of electric force lost per min. to that which remains in the body.
First Experiment, 28th May.							
1st Exper.	10h 0' 0''	30°	150	$2\frac{1}{2}$'	30	165	$\frac{1}{14}$
2d ...	10 2 30	30	120	$5\frac{1}{2}$	40	130	$\frac{1}{18}$
3d ...	10 8 0	30	80	5	20	100	$\frac{1}{25}$
4th ...	10 13 0	30	60	$16\frac{1}{2}$	40	70	$\frac{1}{29}$
5th ...	10 29 30	30	20	21	20	40	$\frac{1}{42}$
6th ...	10 50 30	30	0	$16\frac{1}{2}$	10	25	$\frac{1}{41}$
7th ...	11 7 0	30	—10				
Second Experiment, 29th May.							
1st Exper.	7h 34' 0''	30°	150	2' 40''	20	170	$\frac{1}{23}$
2d ...	7 36 40	30	130	4 50	20	150	$\frac{1}{36}$
3d ...	7 41 30	30	110	6 50	20	130	$\frac{1}{44}$
4th ...	7 48 20	30	90	7 25	20	110	$\frac{1}{41}$
5th ..	7 55 45	30	70	11 45	20	90	$\frac{1}{53}$
6th ...	8 7 30	30	50	17 30	20	70	$\frac{1}{61}$
7th ...	8 25 0	30	30	17 30	15	50	$\frac{1}{58}$
8th ...	8 42 30	30	15	22 30	14	36	$\frac{1}{58}$
9th ...	9 5 0	30	3				

Part II. From the inspection of this table, it is obvious that the diminution of the electricity, which is at first much more rapid than it would have been, had it been produced solely by the contact of the air, becomes at last, when the density is diminished to a certain point, precisely the same. It is certain from this that a silk thread 15 inches long, becomes a perfect insulator, when the reciprocal action of the two balls is measured, in the first experiment of the second table, by a torsion of 40°, and below that; for then the loss of electricity is only $\frac{1}{41}$ per minute, the same as it had been observed on the same day, from the contact of air alone. In the second experiment of the 2d table, the silk thread became a perfect insulator when the repulsive action of the two balls was 70°, and below; for then the loss of electricity was only $\frac{1}{80}$ per minute, as it had been found the same day from the contact of the air alone.

CHAPTER IV.

OF THE LAW ACCORDING TO WHICH ELECTRICAL ATTRACTIONS AND REPULSIONS VARY.

This law was first investigated by Coulomb in 1785. By a set of experiments with his electrical balance, he showed that two small balls charged with the same kind of electricity repel each other with a force varying inversely as the square of their distances from each other. He demonstrated also by experiments conducted with the same apparatus, that two balls charged with different kinds of electricity, attract each other with forces varying inversely as the squares of their distances from each other.*

Experiments on electrical repulsions.

1. The following are the experiments by which Coulomb demonstrated the law of electrical repulsion:—

* Mem. Paris, 1788, p. 572.

(1.) Having electrified the two balls of the balance by means of the head of a large pin, the index of the micrometer corresponding with 0, the ball *a* of the needle separated from the ball *t*, 36 degrees. (See fig. 1.) p. 337. Chap. IV.

(2.) Having twisted the suspending wire 126 degrees, the two balls approached each other, and remained at 18 degrees' distance.

(3.) The suspending wire being twisted 5·67 degrees, the two balls approached within $8\frac{1}{2}$ degrees of each other.

The two balls before being electrified touched each other, and the centre of the ball, *a*, attached to the needle, is not farther distant from the point where the torsion of the suspending wire is 0, than half the diameter of the two balls. The silver wire which suspended the needle was 28 inches long, and it was so fine that its weight was only $\frac{1}{16}$th of a grain. And Coulomb demonstrated that, to twist this wire 360 degrees the force requisite at the point *a*, with a lever *a*, *n* of four inches long is only $\frac{1}{340}$th of a grain: hence, as the forces of torsion are as the angles of the torsion, the least repulsive force between the two balls, separates them sensibly from each other.

In the first experiment, the index of the micrometer being at 0, the balls separated 36°. This occasioned a torsion of $36° = \frac{1}{3400}$th of a grain. In the second experiment the distance of the balls was 18°, but as the micrometer was turned 126°, it follows that at the distance of 18° the repulsive force was 144°. Thus when the distance is reduced to one half, the repulsive force is quadrupled. In the third experiment, the suspending wire was twisted 567°, and the two balls were reduced to the distance of 8°·5 from each other. Here the total torsion was 576°, or four times as much as in the second experiment; and there is only half a degree wanting to render the distance of the balls in the third experiment, one half of what it was in the second. Thus it follows from these three experiments, that the repulsive forces exercised upon each other, by two balls charged with the same kind of electricity, are inversely as the squares of the distances. Varies as $\frac{1}{d^2}$.

The electricity of the two balls diminishes a little during

Part II. the continuance of the experiments. Coulomb ascertained that on the day on which the experiments were made, the electrified balls being in consequence of their repulsion 30° distant from each other, under an angle of torsion of 50°, approached each other 1° in three minutes. As he took only two minutes to make the three preceding experiments, the error resulting from the loss of electricity may be neglected. But when a still greater degree of precision is required, or when the day is moist, the best way of proceeding is, by a preliminary experiment to determine the loss of electricity per minute, and then to employ this observation to correct the result of the experiments made on that day.

The distance of the two balls when they are separated from each other by their mutual repulsion, is not accurately measured by the angle which they form, but by the chord of the arc which joins their centres. The lever at the extremity of which the action is exercised, is not measured by half the length of the needle, or by the radius; but by the cosine of half the angle formed by the distance of the two balls. These two quantities, one of which is smaller than the arc, and diminishes of course the distance measured by that arc, while the other diminishes the lever, compensate each other in some respects. When the arcs are small as in the preceding experiments, we may take them as giving the distance without sensible error. In other cases a rigorous calculation must be made.

2. The balance did not answer so well for determining the law of the attractions of bodies charged with different kinds of electricity: because the balls, when brought within a certain distance of each other, approach and come into contact, which prevented the attracting force at different distances from being observed. Coulomb therefore determined the attractive force by bringing a small ball charged with electricity, within a certain distance of a sphere excited by the opposite electricity. The attractive force was measured by the number of vibrations in a given time, in the same way as the force of gravitation in different parts of the earth's surface, is measured by the number of vibrations of a pendulum of a given length. The apparatus employed in these experiments was the following:—

Mode of determining the law of electrical attractions.

Fig. 1.

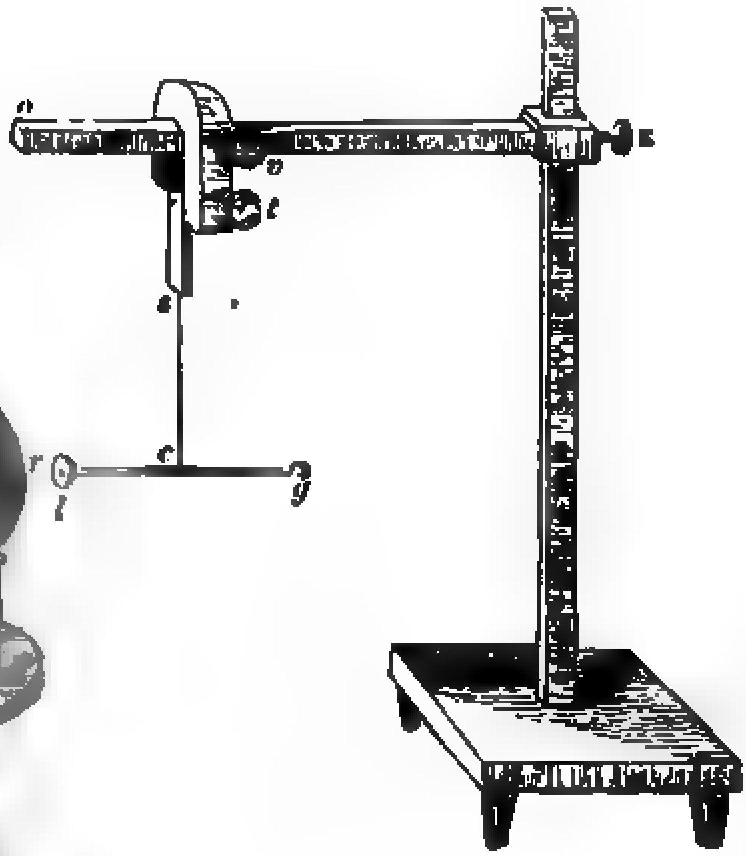
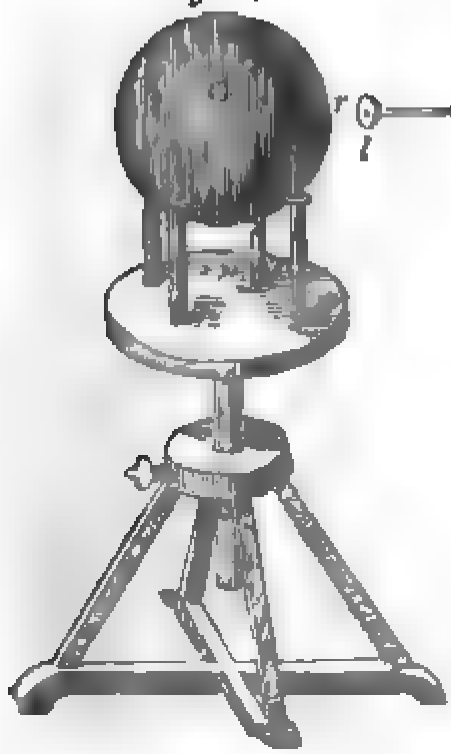


Fig. 2.



The needle, *l*, *g*, (fig. 1), of gum lac, was suspended by the raw silk thread, *s*, *c*, seven or eight inches long. At the extremity, *l*, was fixed perpendicularly to the needle a small circle 8 or 10 lines in diameter, but very light and formed from a sheet of gilt paper. The silk thread is fixed at *s*, to the lower extremity of a slip of wood dried in the oven, and covered with a coating of lac varnish. This piece of wood is fixed by screws at *t*, to a frame which moves along the rod, *o*, E, and which may be stopped at pleasure by means of the screw, *v*.

G (fig. 2), is a globe of copper or pasteboard, covered by tin-foil supported by four pillars of glass coated over with lac varnish, and having the upper extremities of each, (that the insulation may be more complete) terminating in a stick of sealing-wax three or four inches long. These four pillars are

Part II. fixed below in a piece of wood which is placed on a small table, which may be elevated and lowered at pleasure and fixed at the requisite height for the experiment. The rod *o* E may also, by means of the screw E, be fixed at any height required.

Things being thus prepared, the globe G, was so placed that its horizontal diameter G, *r*, corresponded with the centre of the disc *l*, distant from it some inches. A spark was given to the globe by means of a Leyden bottle, while at the same time a conducting body was presented to the disc *l*. The consequence of this was evidently that the disc became charged with an electricity different from that of the globe G: hence when the conductor was withdrawn, the globe and the disc mutually attracted each other.

The globe G, was a foot in diameter, while that of the disc *l*, was seven lines; the needle *l g*, was 15 lines long. The silk thread *s c*, was a single fibre, such as is spun by the silk-worm 8 lines long. When the frame (which slid along *o* E,) was at *o*, the disc *l*, touched the globe at *r*, and in proportion as the frame was drawn towards E, the disc was withdrawn from the centre by quantities given by the divisions 0, 3, 6, 9, 12 inches, (marked upon the leg *o* E, but not given in the figure,) the globe was electrified positively, and of consequence the disc negatively. The result of the experiments was as follows:—

(1.) The disc *l*, being 3 inches from the surface of the globe, or 9 nine inches from its centre, gave 15 oscillations in 20″.

(2.) The disc being 18 inches from the centre of the globe, gave 15 oscillations in 40″.

(3.) The disc being 24 inches from the centre of the globe, gave 15 oscillations in 60″.

Let us see what consequences may be deduced from these three experiments.

It is well known that when all the points of a spherical surface act by an attracting or repelling force inversely as the square of the distance, on a point placed at some distance from that surface, the action is the same as if the whole spherical surface was concentrated in the centre of the sphere. As in the preceding experiments, the disc *l*, was only 7 lines in

diameter, and was never less than 9 inches from the centre of the sphere, we may without sensible error suppose all the lines which go from the centre of the sphere to a point of the disc, parallel and equal. Consequently the whole action of the disc may, like that of the globe, be considered as united to its centre. So that in the small oscillations of the needle, the action which makes the needle oscillate will be a constant quantity for a given distance, and it will act in the direction of the line which joins the two centres. If we call φ the force, and T, the time of a certain number of oscillations, we will have T, proportional to $\frac{1}{\sqrt{\varphi}}$. But if d is the distance G, l, from the centre of the globe to the centre of the disc, and if the attracting forces be inversely as the square of the distance or as $\frac{1}{d^2}$, it will follow that T, will be proportional to d, or to the distance: hence, in the preceding experiments, if the attractive force varies inversely as the square of the distance, the time of the same number of oscillations should be as the distance from the centre of the disc to the centre of the globe. Let us compare the experiments with the theory:—

	Distances.	
1st Experiment .	9 inches .	20″
2d do. .	18 do. .	40″
3d do. .	24 do. .	60″

The distances are as . .	3,	6,	8
The times as . . .	20,	40,	60
By theory they should be .	20,	40,	54

The numbers in the first and second experiments correspond with the theory. When we compare the third with the first, the discordancy amounts to $\frac{1}{10}$th. But it is necessary to remark that these three experiments occupied about 4 minutes. Upon the day of the experiments, the loss of electricity per minute was $\frac{1}{40}$th. Now, Coulomb demonstrated that when the density of the electricity is not very great, the electric action of two electrified bodies diminishes in a given time exactly as the electric density: hence, as the experiments lasted 4′, and the loss was $\frac{1}{40}$ per minute, the intensity must have been diminished

Varies as $\frac{1}{d^2}$.

Part II. (independent of distance) about $\frac{1}{30}$th: hence to have the corrected time of the 15 oscillations in the last experiments, we must say $\sqrt{10} : \sqrt{9} :: 60''$: quantity sought $= 57''$. This differs by only $\frac{1}{20}$th from the number 60″ found by experiment.

Thus it appears that the electrical attractions as well as repulsions vary inversely as the square of the distance.

The experiments of Coulomb have been repeated and confirmed by Mr Snow Harris.* But in a subsequent set of experiments he found that the attractions and repulsions sometimes varied as $\frac{1}{d^2}$, and sometimes as $\frac{1}{d}$; and in consequence he throws some doubts on the accuracy of Coulomb's experiments;† but he does not appear to have seen Coulomb's original experiments, but merely the abstract of them given by Biot in his *Traité de Physique.*

Lord Stanhope's experiments on electric atmospheres.

The late Earl Stanhope showed by an ingenious set of experiments, which he published in the year 1779, that the density of electricity in the electrical atmosphere surrounding an excited body, diminishes inversely as the square of the distance from the charged body. As this in fact comes to the same thing as the demonstration of the rate at which electrical attractions and repulsions vary, made by Coulomb seven years later, or in the year 1785, electricity is really indebted to this nobleman for this important discovery. On this account it will be proper here to point out the steps by which his Lordship was led to the conclusion just stated.

It had been already ascertained by Canton, that an electrified body communicates its own electricity to all the particles of air which come in contact with it. These particles are immediately repelled and their place supplied by a new set of aerial particles. The consequence of this must be, that the air immediately surrounding an excited body must be also excited, and must possess the same kind of electricity with the excited body.

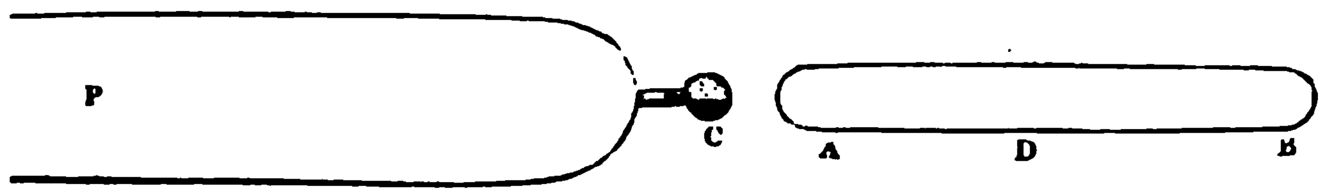
It is obvious that the electrical density of this air must diminish according to its distance from the surface of the excited

* Phil. Trans. 1834, p. 238. † Ibid. 1836, p. 433.

body: hence according to Lord Stanhope, the reason why bodies charged with the same kind of electricity *repel* each other. They must move to those parts of their atmospheres where the electricity is *least*. Bodies excited with different kinds of electricity, on the contrary, approach each other, because each moves towards the extremity of its electrical atmosphere.

The reason why points part with electricity so easily, is that they penetrate into a much less dense electrical atmosphere: hence the resistance to the escape of electricity is small.

When a body is placed within the electrical atmosphere of another, but beyond the striking distance, the end next the charged body, assumes an opposite electricity, while the end farthest remote assumes the same kind of electricity with the excited body, and there will be a neutral point somewhere between these two extremities.



Let P C, be a charged body, and let A B, be a brass cylinder terminating in hemispheres, placed within the electrical atmosphere of P C, but so far from it that a spark cannot pass from C to A; and let A B, be insulated. Let P C, be charged with positive electricity. The extremity A, will be charged with negative electricity, while the extremity B, will be charged with positive electricity. Between A and B, there will be a point, D, which is neutral, or which exhibits no phenomena either of vitreous or resinous electricity. Now, Lord Stanhope showed by numerous experiments, that the place of the point, D, varies with the distance of A from C. And that it is always a fourth point of an harmonical division of the line, B A C, the other three points, C, A, B, being given.

It is well known to mathematicians, that three quantities are said to be in harmonical ratio, when the first is to the third, as the first minus the second is to the second minus the

[illegible] —

[illegible]

[illegible] C D [illegible]

[illegible]

Thus it appears, that when the distance between C and A is 36 inches, and the cylinder, A B, 44 inches, the neutral point, D, is situated [illegible] inches from the extremity A, and 29[illegible] inches from the extremity B.

The following table exhibits the result of the experiments made by Lord Stanhope, A B, being placed at different distances from the prime conductor, P C. The length of A B was always 40 inches:—

Length of A C in inches.	Length of A D in inches.	Proportion of A D to A B.
4	$\frac{4 \times 40}{48} = 3\frac{1}{3}$	$\frac{1}{12}$ or $\frac{1}{12}$
8	$\frac{8 \times 40}{56} = 5[illegible]$	$\frac{2}{14}$ or $\frac{1}{7}$
12	$\frac{12 \times 40}{64} = 7\frac{1}{2}$	$\frac{3}{16}$ or $\frac{1}{5\frac{1}{3}}$
16	$\frac{16 \times 40}{72} = 8\frac{8}{9}$	$\frac{4}{18}$ or $\frac{1}{4\frac{1}{2}}$
20	$\frac{20 \times 40}{80} = 10$	$\frac{5}{20}$ or $\frac{1}{4}$
24	$\frac{24 \times 40}{88} = 10\frac{10}{11}$	$\frac{6}{22}$ or $\frac{1}{3\frac{2}{3}}$
28	$\frac{28 \times 40}{96} = 11\frac{2}{3}$	$\frac{7}{24}$ or $\frac{1}{3\frac{3}{7}}$
32	$\frac{32 \times 40}{104} = 12\frac{4}{13}$	$\frac{8}{26}$ or $\frac{1}{3\frac{1}{4}}$

Length of A C, in inches.	Length of A D, in inches.	Proportion of A D, to A B.
36	$\frac{36 \times 40}{112} = 12\frac{6}{7}$	$\frac{9}{28}$ or $\frac{1}{3\frac{1}{9}}$
40	$\frac{40 \times 40}{120} = 13\frac{1}{3}$	$\frac{10}{30}$ or $\frac{1}{3}$
44	$\frac{44 \times 40}{128} = 13\frac{3}{4}$	$\frac{11}{32}$ or $\frac{1}{2\frac{10}{11}}$
48	$\frac{48 \times 40}{136} = 14\frac{2}{17}$	$\frac{12}{34}$ or $\frac{1}{2\frac{5}{6}}$

These results show that the point D, is always such that the lines C B, C D, C A, are in harmonic proportion, the points C, A, and B, being given. To obtain them it is necessary that the air should be dry. Should there be any moisture in the atmosphere to carry off the electricity from A B, the situation of D, will no longer be such as to divide the line C B, in the harmonic proportion.

It is obvious enough that the two limits of the point D, are the *middle* of the line A B, and the *nearest* extremity A. It will be in the middle of the line A, B, when the body A B, is at an infinite distance from P C. It will be in A, when the body A B, touches P C.

It is clear that the position of D, must be always such, that the plane passing through it, will divide the electricity of the body A B, into two equal parts, if the density of the electricity of the electrical atmosphere diminished inversely as the simple distance from the charged body P C. Then the whole of the electricity of this atmosphere round A B, would be represented by the hyperbolical area (see frontispiece) A N Q B, contained between the equilateral hyperbola N R Q, the asymptote A B, from the centre C, and the ordinates A N, and B Q. And if the electrical atmosphere had its electricity diminished according to that law, the point D, would be found by taking the ordinate D R, a mean proportional between A N, and B Q. But if the electricity of the electric atmosphere diminishes inversely as the squares of the distance from P C, then this electricity round A B, will be represented by the hyperbolical solid *n* N Q *q*, formed by the revolutions of the hyperbolical space A N Z W, round the asymptote C W.

Part II. This space is bounded by the portion of the equilateral hyperbola intercepted between the ordinates A N, B Q. It is a known property of this hyperbolical solid *n* N, Z *z*, that the area of any perpendicular section represented by *n* A N, will be to the area of any other perpendicular section, *r* D R, *q* B Q, inversely as the squares of their respective distances, (A C, C D, C B,) from the centre C of the hyperbola: hence the quantity of electricity superinduced upon the curved surface of the insulated cylindrical body A B, at the distances C A, C D, C B, from the charged body P C, will be exactly proportional to the areas of the circular sections represented by *n* A N, *r* D R, *q* B Q, &c. Now, to find the position of the point D, we have to find the point in the asymptote C W, through which a plane *r* D R, being passed, will divide the hyperbolic solid *n* N Q *q*, into two equal parts. That plane will obviously determine the position of D.

Now, to divide the hyperbolical solid into two equal halves, we have only to take D R, an arithmetical mean between the two ordinates, A N, B Q; or the point, D, is such that the ordinate, D R, is equal to half the sum of the two ordinates, A N, B Q. Or which is the same thing, (from the nature of the hyperbola), we must take the hyberbolical abscisse, C D, a mean harmonical proportion between the two abscisses, C B, and C A. That is, we must take the point, D, so that the whole line, C B, is to its extreme part, C A, as the other extreme part, B D, is to the middle part, D A. For it is a well known proposition, that the reciprocals of quantities which are in arithmetical progression, will be in harmonical progression.

That the plane, R D *r*, will divide the solid *n* N Q *q* into two equal parts may be demonstrated in the following manner:—

Let *p* denote the ratio of the circumference of a circle to its diameter, or (which is the same thing) the ratio of the area of a circle, to the square of its radius.

Then the cylinder, *r* V N *n*, (whose height is A C, and the radius of whose base is N A,) will evidently be equal to $p \times$ N A $\times$ N A $\times$ A C.

The cylinder $x\,X\,R\,r$ will be equal to $p \times R\,D \times R\,D \times C\,D$.

And the cylinder $y\,Y\,Q\,q = p \times Q\,B \times Q\,B \times C\,B$.

Now, from the nature of the hyperbola, every rectangle under any abscisse, and its correspondent ordinate will be a given quantity, which we shall call $c\,\underline{c}$. Therefore,

$$N\,A \times A\,C = R\,D \times D\,C = Q\,B \times B\,C = c\,\underline{c}.$$

And the cylinder $v\,V\,N\,n = p \times c\,c \times N\,A$.
$x\,X\,R\,r = p \times c\,c \times R\,D$.
$y\,Y\,Q\,q = p \times c\,c \times Q\,B$.

Therefore, (dividing by the common quantity, $p \times c\,c$) the cylinders $v\,V\,N\,n$, $x\,X\,R\,r$, $y\,Y\,Q\,q$, are exactly in the same ratio to each other, as the ordinates N A, R D, and Q B, respectively.

Now, R D is an arithmetical mean between A N and B Q, therefore the cylinder $x\,X\,R\,r$, is an arithmetical mean between the cylinders $v\,V\,N\,n$ and $y\,Y\,Q\,q$. That is to say, $v\,V\,N\,n - x\,X\,R\,r = x\,X\,R\,r - y\,Y\,Q\,q$.

But it has been demonstrated that every infinite acute hyperbolic solid, is equal to the cylinder whose base is the same with that of the solid, and whose height is equal to the distance between that base and the centre of the hyperbola. Therefore,

$v\,V\,N\,n =$ infinite acute hyperbolic solid $n\,N\,Z\,z$.
$x\,X\,R\,r = r\,R\,Z\,z$.
$y\,Y\,Q\,q = q\,Q\,Z\,z$.

Consequently,
$v\,V\,N\,n - x\,X\,R\,r = n\,N\,Z\,z - r\,R\,Z\,z = n\,N\,R\,r$ and $x\,X\,R\,r - y\,Y\,Q\,q = r\,R\,Z\,z - q\,Q\,Z\,z = r\,R\,Q\,q$.

Hence it follows that $n\,N\,R\,r = r\,R\,Q\,q$.

Thus it follows evidently from the experiments of Lord Stanhope, that the density of the electricity of electrical atmospheres diminishes inversely as the square of the distance from the excited body.

CHAPTER V.

OF THE DISTRIBUTION OF ELECTRICITY ON BODIES.

COULOMB demonstrated that it is a consequence of the law, that the particles of electricity repel each other inversely as the square of their distance, that the electricity when accumulated in a conducting body is distributed totally on the surface of the body, and that none of it exists in the interior of the body.* He showed likewise the truth of this law experimentally.

Coulomb's electrometer. The electrometer which he employed for that purpose was exceedingly delicate. It was made in the following manner. A thread of gum lac was drawn at the flame of a candle about the thickness of a stout hair, and from ten to twelve lines in length. One of its extremities was attached to the thick end of a small pin deprived of the head. This pin was suspended by a thread of raw silk, such as it is spun by the silk worm. To the other extremity of the gum lac thread was fixed a circle of tinsel, about two lines in diameter. The whole of this electrometer was suspended in a glass cylinder to prevent it from being affected by currents of air. Its sensibility was such that a force amounting only to the $\frac{1}{1000}$th part of a grain, made it move more than 90 degrees.

To this electrometer a small degree of the same electricity was given as that of the body to be tried. The substance subjected to the experiment was a cylinder of solid wood, four inches in diameter, pierced by several holes of four lines in diameter, and four lines in depth. This cylinder was placed upon a non-conductor, and several electric sparks were given to it either by means of a Leyden phial, or the plate of an electrophorus. At the extremity of a small cylinder of gum

* Mem. Paris, 1786, p. 75.

lac of a line in diameter, was insulated a small circle of gilt paper of a line and a half in diameter.

Electricity confined to the surface of bodies.

This little circle of gilt paper was made to touch the surface of the excited wooden cylinder. It was then presented to the electrometer, previously excited slightly with the same kind of electricity as the wooden cylinder. The electrometer was forcibly repelled. It was then introduced into one of the holes in the wooden cylinder and brought in contact with the bottom, taking care not to touch the sides of the hole, nor the surface of the cylinder of wood. When presented to the electrometer, it either gave no signs of possessing any electricity, or gave feeble symptoms of containing an electricity different from that of the cylinder of wood. The reason of this opposite electricity, which was only occasionally observed, was, that when the little cylinder of gum lac is introduced into the hole, the electrical action of the surface of the excited body communicates to the gum lac, a small quantity of an electricity different from its own, because this small thread of gum lac is insulated within the sphere of its activity. The proof that this explanation is satisfactory, is, that if we touch the gilt paper circle we do not destroy the small quantity of electricity which it possesses, showing that this electricity must be lodged in the lac, and not in the gilt paper.*

We may take a hollow sphere of metal, having in it a hole of a few lines in diameter, place it upon a non-conductor, and communicate electricity to it by means of an electrical machine. When the sphere is thus excited, we may introduce into its interior a small metallic globule attached to the extremity of a gum lac thread. Upon withdrawing this metallic globule, and presenting it to an electrometer we shall find that it has no electricity whatever, or that it has (occasionally for the reason already explained) a feeble electricity of the opposite nature from that of the metallic sphere.

Thus it appears that the quantity of electricity accumulated in a body, depends not upon the quantity of matter in the body, but upon the extent of its surface; and that hollow bodies,

* Mem. Paris, 1786, p. 72.

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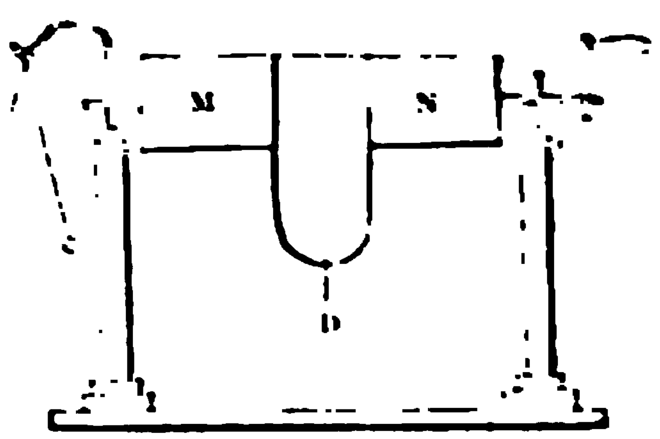
however thin, may be charged with just as much electricity as bodies of the same surface, and qutie filled with matter. Advantage is taken of this important fact in constructing the prime conductors of electrical machines, which arealways made hollow and thin for the sake of lightness.

Experimental proof of this.

There is a curious enough experiment, which serves to show that electricity is confined to the surface of bodies, and which therefore may be mentioned here. Let M N, be an insulated conductor, moveable on an axis. Let a metallic ribbon be rolled on this cylinder to the extremity of which is attached a silk thread D. To this conductor let a sensible electrometer be attached, composed of two metal or linen threads, to the extremities of which two pith ball sphericles are attached. When electricity is communicated to this conductor, the pith balls immediately repel each other. Unroll the ribbon by pulling the silk thread D; in proportion as the ribbon is unrolled, the pith balls approach, and if the weather be dry, all signs of electricity disappear when the ribbon is unrolled; and the balls begin anew to separate, when the ribbon is again rolled on, and diverge as far as ever when matters are restored to their original state.



Electricity, then, is all deposited on the surface of bodies, and the only obstacle to its leaving that surface and being instantly dissipated, is the pressure of the atmosphere.*

Electricity proportional to the surfaces.

Coulomb proved by very simple but convincing experiments, that electricity deposites itself on bodies according to their surface, that it has no affinity or attraction for one body more

* Strictly speaking there is another obstacle. It is the difficulty with which electricity passes along certain bodies. This seems to be owing to something like an adhesion between the surface of the body and the fluid. This view of the subject is strengthened by the well known fact, that the conducting power diminishes with the intensity of the electricity. This adhesion must to a certain extent be similar to attraction, and it is probably the great cause of the attractions and repulsions between excited bodies.

than another; but that if two bodies having the same surface be placed in contact, whatever their nature may be, they will divide the electricity equally between them. With this distinction, depending upon the unknown cause why some bodies are good and some bad conductors, that if a good conductor touch an excited body of the same surface, it will instantly take possession of one-half of its surplus electricity, whereas a bad conductor requires to be left a little while in contact with the excited body, to be able to assume its proper quantity.*

When the excited body is a sphere, the thickness of the electric coating ought, from reasons of symmetry, to be everywhere equal. And this conclusion is conformable to experiment.

When additional quantities of electricity are accumulated upon the surface of a sphere, it comes to the same thing, as far as the experimental investigation is concerned, whether the thickness of the coating of electricity be increased, or whether, the thickness remaining the same, the density of the electricity augments on each point of the surface. The first supposition is more conformable to our notions of a fluid than the second.

Coulomb made a set of experiments, to determine the way in which electricity is distributed on the surface of bodies of various shapes.† His method was to apply a small gilt paper disc insulated on a thread of gum lac, to different parts of the excited body in succession, and to measure, by means of his balance, the quantity of electricity with which it became charged. He proved by a set of experiments equally simple and decisive, that the quantity of electricity with which the disc became charged, was sensibly the same with that of the point of the surface of the excited body with which it had been placed in contact, and that when the electricity of an excited

* Mem. Paris, 1786, p. 69.

† The best account of these experiments which I have seen, is given by Biot in his Traité de Physique, ii. 263. Biot seems to have derived his information from Coulomb's manuscripts. For I am not aware that they were ever published by their author, owing doubtless to the state of France at the beginning of the Revolution.

body, is either augmented or diminished, the variations on every part of the surface are proportional to the quantity of electricity already present. Thus, if on one part of a surface the electricity be twice as great as upon another part, if we double the charge or reduce the charge to one-half, the first part of the surface will receive twice as much, or part with twice as much as the other part.

Coulomb showed also, that the quantity of electricity with which the gilt paper disc becomes charged, is sensibly the same with that of the body which it is made to touch, and that when two bodies are brought into contact, the electricity contained in them is sensibly divided in proportion to the surface of each. He insulated a globe of 8 inches diameter, and charged it with positive electricity, as he did also the moveable ball of the balance. He then touched this globe with another whose diameter was only 1 inch; and applied this little globe to his balance. A torsion of 144° was necessary to balance the repulsive force at a fixed distance. He next touched the surface of the large globe in a tangent direction with a circular plane, isolated, and having a diameter of 16 inches. Having removed this plane, he repeated the experiment with a small globe. A torsion of 47° was now sufficient to balance the repulsive force.

To understand the consequences to be drawn from this experiment, we must recollect that the surface of the great globe was equal to that of 4 great circles, each 8 inches in diameter: each face of the circular plane of 16 inches diameter, was equal to 4 circles of 8 inches in diameter, and as the plane has two faces, its surface was exactly double that of the sphere. The electricity in the great globe before it was touched by the plane may be represented by 144, and after it was touched by it by 47: hence, the electricity carried off by the plane, was obviously $144 - 47 = 97$. So that the plane carried off 97, while the globe retained 47. But 97 is very nearly double 47. Thus we see that the electricity was divided between these two bodies in proportion to their surfaces.

In determining the quantity of electricity on different parts of the surface of a body, we must take care that the gum lac, to which the gilt paper disc is attached, be not a conductor;

because in that case, electricity would accumulate in it, and vitiate all the results. In general, the lac which has the darkest colour is the best for the purpose. But the threads ought to be examined by bringing them near a charged conductor, and then presenting them at an electrometer. All those which exhibit any symptoms of electricity, ought to be rejected.

Distribution on a steel plate.

Coulomb determined the distribution of electricity upon a steel plate, 11 inches long, 1 inch broad, and half a line thick. It was insulated and charged with electricity. To be able to touch it over all its breadth, the gilt paper disc was made an inch long, and three lines broad. He first applied the disc to

C

, the centre of the plate, at C, then at 1 inch from its extremity, and he obtained the following results :—

	Torsions observed.	Mean torsions in the middle.	Ditto 1 inch from end.	Ratio.
Touched in the middle,	370°			
At 1 inch from end,	440	360°	440°	1·22
In the middle,	350	350	417·5	1·20
At 1 inch from end,	395	335	395	1·18
In the middle,	320			
			Mean	1·10

That is to say, if the electricity at the centre of the plate be represented by 1, that at an inch from the end will be represented by 1·2. So that they are nearly equal.

The experiment was repeated with this difference, that the gilt paper disc was applied to the very end of the plate, but still entirely on the surface of it. The results were as follows :

	Torsions observed.	Mean torsions in the middle.	Ditto at the end.	Ratio.
Touched at the end,	400°			
In the middle,	195	195°	395°	2·02
At the end,	390	190	390	2·05
In the middle,	185	185	370	2·00
At the end,	350			
			Mean	2·02

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That is to say, if the quantity of electricity in the middle of the plate be 1, that at the extremity will be 2·02, or very nearly double.

The experiment was repeated a third time, but the disc instead of being applied at the extremity of the plate, was applied to the edge D, so as to be equally in contact with both faces. The result was as follows:—

	Torsions observed.	Mean torsions in the middle.	Mean torsions on the edge.	Ratio.
Touched in the middle,	305°			
On the edge,	1175	295°	1175°	3·98
In the middle,	285	285	1156	4·05
On the edge,	1137			
			Mean	4·01

That is to say, if the electricity in the middle be 1, that at the edge is 4·01, or four times as great. So that it contains the accumulated electricity of the extremities of each side of the plate.

These experiments were repeated with a plate of steel, twice as long as the preceding, but agreeing with it in all its other dimensions. The result was exactly the same.

It is obvious from these experiments, that when the gilt paper disc is applied to one of the sides of the steel plate, it only imbibes the electricity of that side, and not of the other. We see too that the electricity is almost uniformly diffused upon the surface of the plate; since the increase does not become sensible till we approach within an inch of the end, and that the length or shortness of the plate has no effect upon this distribution.

Distribution on a cylinder.

The rapid augmentation of electricity towards the extremities of plates, is not peculiar to them. It takes place equally in all long prismatic or cylindrical bodies. And the smaller the diameter of these bodies is in proportion to their length, the more rapid is their augmentation. Coulomb insulated a cylinder of 2 inches in diameter, and 30 inches in length, terminated by two hemispheres. By experiments similar to

those just stated, he found the ratios of electricity on different parts of this cylinder as follows:—

At the centre	1
At 2 inches from end . .	1·25
At 1 inch from end . . .	1·80
At the centre of the end . .	2·30

When the cylinder becomes smaller as we approach the extremity, the increase of electricity becomes still more rapid. And when the extremity becomes elongated like the point of a cone, the accumulation at that extremity becomes so great, that the resistance of the air is not sufficient to retain it, and it escapes, producing sparks of light which are visible in the dark.

In these bodies the uniform distribution of electricity extends to within a very small distance of the extremity. It is clear that such bodies ought to lose their charge of electricity with great rapidity, as is known from experiment to be the case.

Distribution on circular plates.

Coulomb made analogous experiments on circular plates. The following table shows the result of one of these trials:—

Distance from the edge.	Electricity observed.
5 inches (centre of plate) .	1
4	1·001
3	1·005
2	1·17
1	1·52
0·5	2·07
0	2·90

If we represent the distances from the edge by x, and the electrical intensities by y, Biot has shown that the relations of these to each other may be represented by the following empirical formula:—

$$y = 1 + A\,(\mu^{x} - \mu^{2r-x})$$

r being the radius of the plate, and A and μ two constants to be determined from observation. We obtain A by making $x = o$, and substituting for y, the value found, as in the table. We have,

$$2{\cdot}9 = 1 + A\,(1 - \mu^{2r})$$

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If we take 1 inch for the unity of length, $2r$ will be equal to 10. Now, as μ is very nearly 0·3, its tenth power is so small a quantity that it may be neglected. We have $2{\cdot}9 = 1 + A$, and $A = 1{\cdot}9$.

To determine μ let us make $x = 1$, and substitute for y its value 1·52. In this case $\mu^{2r-x} = \mu^9$ a quantity too small to be sensible. We have, therefore,

$$1{\cdot}52 = 1 + 1{\cdot}9\,\mu \text{ or}$$

$$\mu = \frac{0{\cdot}52}{1{\cdot}9} = 0{\cdot}27.$$

But the calculation agrees better with experiment, if we make $\mu = 0{\cdot}3$.

Why electricity is dissipated by points.

This subject attracted the attention of M. Poisson, who applied to it all the resources of the most refined calculus, and determined the thickness of the coating of electricity upon bodies of different forms from the hypothesis that positive and negative electricity are two fluids, the particles of each of which repel each other with forces varying inversely as the square of the distance; while the vitreous electricity attracts the resinous with forces varying according to the same law.* He showed that the exterior surface of the electrical coating coincides with that of the body, and that as the coating is very thin, the interior surface is but little distant from it. In a sphere both the exterior and interior surfaces are spherical, and the centre of these surfaces is the same with that of the centre of the body.

In an ellipsoid the thickness of the coating at the extremities of the two axes is proportional to the relative lengths of the axes.

The electric coating acts by attraction and repulsion on the particles of electricity, situated beyond the surface of the body, or on the surface of the body itself. It attracts them if of a different nature, and repels them if of the same nature. This repulsion is exercised against the particles of the air, which repels the electricity in its turn, and keeps it in its place. The repulsion exercised by the electrical coating is as the

* See Mem. de l'Institut, 1811.

square of its thickness. It is therefore variable at different points of the body, unless that body be spherical. And as it increases as the square of the thickness, and as this thickness is proportional to the excess of the diameter of the body in one direction above that in another, it may easily overcome the pressure of the atmosphere, and thus enable the electricity to make its escape: hence the reason of the effect of points in discharging bodies charged with electricity. M. Poisson has shown that the repulsive force of the electricity at the summit of a cone, if it could accumulate on it, would be infinite.

Mutual action of excited bodies on each other.

When several excited bodies are placed in the neighbourhood of each other, they produce certain effects upon the state of the electricity in each. These effects have been also subjected to calculation by M. Poisson. His analytical investigations were founded upon the following general principle which he first announced:—

" When several excited bodies are brought into the neighbourhood of each other, and arrive at a permanent electrical state, it is necessary that the resultants of the actions of the electrical coatings which cover them upon any point in the interior of any one of these bodies, should be nothing. For if this was not the case, the combined electricity in the point under consideration would be decomposed, and the electrical state would change, contrary to the hypothesis of its permanence."

This principle furnishes at once as many equations as there are bodies under consideration, and as the problem presents of unknown quantities. But the solution of these equations frequently surpasses the present state of analytical investigation. But M. Poisson has succeeded in overcoming all the analytical difficulties in the case of two spheres, placed in contact, or in the neighbourhood of each other, and charged each with any quantity of electricity.

When the two spheres are equal, and in contact, and charged with electricity, whether positive or negative, calculation shows that there is no free electricity at the point of contact of the two spheres. From that point the coating of electricity augments in thickness as its distance increases from the point of

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contact, and it always attains its maximum at 180° of distance, or at the other end of the diameter that passes through the point of contact of the two spheres. The rate at which the thickness of the coating increases depends upon the relative lengths of the diameters of the two spheres.

Experiments on this subject were made by Coulomb in the way already described in the former part of this chapter. These experiments were compared by Poisson with the result of his calculations, and found to accord with them very closely. The following table shows the relative thickness of the coating of electricity upon two equal globes at different distances from the point of contact, as determined experimentally by Coulomb, and by calculation by Poisson:—

Distribution of electricity on two equal globes.

	Thickness of coating of electricity as determined by	
	Coulomb.	Poisson.
Point of contact	0	0
20°	Insensible	Insensible
30	0·2083	0·1707
60	0·7994	0·7452
90	1·0000	1·0000
180	1·0576	1·1400

The following table exhibits the results of experiment and calculation with two unequal spheres, the diameters of which were to each other as 1 to 2. The observations apply to the smaller sphere:—

On two unequal globes.

Position of the points compared.	Proportional thickness of coating	
	Coulomb.	Poisson.
30°	Insensible	Insensible
60	0·5882	0·5563
90	1·0000	1·0000
180	1·3333	1·3535

We see that in this case the thickness of the coating in the smaller sphere (though insensible to 30°) varies more than when the two spheres are equal. For at 60° and 180° the thicknesses are to each other as 0·5882 to 1·3333, or as 1 to 2¼ ; while in the other case they are to each other as 0·7994 to 1·0576, or as 1 to 1⅓. By a sort of compensation the

greater of the two spheres experiences less variation than in the case of equality. For the thickness of the coating, insensible at 6° or 7°, increases so rapidly that at 30° it is $\frac{7}{8}$ths of what it is at 90°; and from 90° to 108° the variation is insensible.

Coulomb compared the absolute thickness of the electrical coating on the two globes at 90° from the point of contact. In the smaller globe he found it 1·25, in the larger 1. Poisson obtained by calculation 1·238 and 1.

When two globes of different sizes (one charged with electricity) are brought in contact and then separated, the smaller one of the globes is, compared to the other, the smaller is the quantity of electricity which it carries off. But this proportion is very far from being that of the surfaces; for then the coatings of electricity would be of equal thickness in both, whereas it is always thickest in the smaller globe, and the thickness increases as the size of the globe diminishes. But from the slowness of the increase in Coulomb's experiments, there is reason to believe that it does not go on indefinitely.

When the globes after contact are separated.

When the two unequal spheres after being separated from each other, are placed at a certain distance from each other, a very curious phenomenon is observable in the smaller sphere. To explain it intelligibly, let us suppose the electricity of the two spheres to be positive. We have seen that this electricity is null at the point of contact. But the instant we separate the two spheres, the electricity at the point of contact of the smaller sphere becomes negative. This state diminishes in proportion as the smaller globe is carried to a greater distance from the large globe, and at a certain distance, depending on the ratio between the diameters of the two globes, it totally disappears. Then the point of contact of the smaller globe becomes in the same state as when actually in contact. From this position, if we increase the distance, the electricity remains of the same nature over the whole extent of the globe, and of the same kind as during the contact. In the larger globe, the electricity continues always the same as during the contact. In an experiment made by Coulomb, the diameter of the larger globe was 11 inches, and that of the smaller eight inches. While the distance between them was less than 1

Part II.

inch, the point of contact of the smaller globe gave signs of an electricity opposite to that of the large globe. When the distance became 1 inch, the electricity of the point of contact of the smaller globe became null, as when in contact, and at all greater distances, it remained of the same nature as that of the other sphere. When the diameter of the smaller globe was only 4 inches (that of the greater remaining the same), the opposite electricities were sensible to the distance of two inches. When the diameter of the smaller globe was only 2 inches or less, the opposite electricities continued to the distance of 2 inches 5 lines, but not farther.

On the contrary, when the difference between the size of the two globes diminishes, the distance at which the two electricities manifest themselves also diminishes, and it becomes null when the two globes become equal.

These experiments succeed only when the air is very dry. If it be moist, the electricity of the larger globe escaping, neutralizes that in the smaller globe, and renders the phenomena indistinct or quite insensible.

Effect when two excited globes approach.

When two excited spheres are made gradually to approach without having ever been in contact, the thickness of the electrical coat at the points of the two globes immediately opposite to each other, becomes greater and greater, and increases indefinitely as the distance between them diminishes. The pressure on the atmosphere increasing as the square of this thickness, must at last overcome the resistance of the air, and the fluid passing from the one globe to the other, occasions a spark which appears before the two globes come into contact. The electricity is different, though of equal thickness in each globe. If the one is excited with positive, and the other with negative electricity, it is positive in the former and negative in the latter. When both contain the same electricity (positive for example), there takes place a decomposition of the combined electricity in the globe which contains less of the positive electricity than it would have done in case of contact. The negative electricity resulting from this decomposition accumulates at the point opposite the other globe, while this latter globe remains positive throughout its whole extent.

M. Poisson has also subjected to calculation the state of the most remote points of two excited globes, when placed at a certain distance from each other. As the two globes approach, the thickness of the coating of electricity in these points, approaches more and more to what it would be at the instant of contact. But as it arrives at this limit very slowly, the consequence is, that at very small distances the state of these points differs considerably from what it would be, if actual contact or the emission of a spark were to take place.

To these experimental deductions of Coulomb, and calculations of Poisson, may be added the following conclusions determined experimentally by Mr Snow Harris:—

Mr Harris's conclusions.

1. When the surface of a charged jar or conductor is doubled, the force is reduced to $\frac{1}{4}$, when tripled to $\frac{1}{9}$, &c.*

2. The *same* quantity of electricity produces always the *same* heating effect whatever be the tension.

3. When plates of the same area but varying in length, are charged with the same quantity of electricity, the intensity is inversely as the length of the plates, and this, not only with regard to plates, but also with cylinders.

4. When the perimeter is constant, the intensity is inversely as the area.

5. The capacities of a sphere and plane circle of the same area are equal.†

It seems to follow from the experiments of Faraday, that every atom of matter is associated with the same quantity of electricity.‡ If this be so, electricity agrees with heat in this particular.

* Phil. Trans. 1834, p. 219. † Ibid. ‡ Ibid. p. 116.

CHAPTER VI.

OF THE PASSAGE OF ELECTRICITY THROUGH BODIES.

It has been already stated in a former chapter, that bodies, with regard to electricity, may be divided into two sets: namely, those through which it passes with facility, called *conductors;* and those through which it passes with difficulty or not at all, called *non-conductors.* In reality, no body is a perfect non-conductor; for electricity constantly makes its escape at last, whatever the substances are by means of which we attempt to confine it: hence the term *non-conductor* is merely relative. In the third chapter of this treatise will be seen the experiments of Coulomb, showing that electricity makes its escape along the best insulators that can be employed; and that the best insulator or the worst conductor is the *resin of lac.*

Children's experiments on the conducting power of different metals.

The best conductors are the metals; but it has been ascertained that they differ considerably from each other in their conducting power. An unexceptionable set of experiments, on the conducting power of the different metals, was made by Mr Children, in the year 1814. These experiments I was fortunate enough to witness.* Mr Children's battery consisted of 20 pairs of zinc and copper plates, each six feet long, and 2 feet six inches broad, joined together by straps of lead, and plunged into a mixture of nitric and sulphuric acids, diluted with from twenty to forty times their weight of water. This battery, when in full action, rendered a platinum wire 5 feet 6 inches long, and $\frac{11}{100}$ths of an inch in diameter, red hot throughout, so that the ignition was visible in full daylight. 8 feet 6 inches of platinum wire $\frac{44}{100}$ths of an inch in diameter were heated red. A bar of platinum $\frac{1}{6}$th inch square, and 2 inches long, was heated red hot and fused at the end.

* An account of them will be found in Phil. Trans. 1815, p. 363.

round bar of the same metal $\frac{276}{1000}$th of an inch in diameter, and $2\frac{1}{2}$ inches long, was heated bright red throughout. Chap. VI.

To determine the relative conducting powers of different metals, two wires of the metals to be compared were taken of equal diameter and length, one end of each was in contact with one of the basins of mercury, communicating with the poles of the battery, and the other end being bent to an angle, the wires were connected by hooking them to each other. The length of each wire was 8 inches, and its diameter $\frac{1}{30}$th of an inch. The results were as follows:—

1. The wires being platinum and gold, the platinum was instantly ignited, while the gold remained unaffected.

2. Gold and silver. The gold was ignited, the silver not.

3. Gold and copper. Both metals were equally ignited.

4. Gold and iron. The iron was ignited, the gold unchanged.

5. Platinum and iron. The iron ignited instantly next the pole of the battery. Then the platinum became ignited through its whole extent. After this, the iron became more intensely ignited than the platinum, and the ignition of the latter decreased.

6. Platinum and zinc. The platinum was ignited, the zinc was not; but melted at the point of contact.

7. Zinc and iron. The iron was ignited; the zinc bore the heat without fusing.

8. Lead and platinum. The lead fused at the point of contact.

9. Tin and platinum. The tin fused at the point of contact.

10. Zinc and silver. The zinc was ignited before it melted; the silver was not ignited.

From these experiments it has been deduced, that the order of these metals as conductors of electricity is as follows:—

Silver,
Zinc,
{ Gold,
{ Copper,
Iron,
Platinum.

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Tin and lead fuse so immediately at the point of contact, that no conclusion respecting them could be drawn.

Experiments of Mr Harris.

But the most complete set of experiments on the conducting powers of the different metals are those of Mr Harris.* He determined the degree of heat induced into wires of the same size by passing through them the contents of an electric battery charged exactly to the same intensity. This heat was measured by the expansion induced in a given volume of air by the wires. The conducting powers were considered as inversely as these expansions. The following table exhibits the relative heats measured by the rise of a column of spirits (in a tube) pressed upon by the expanded air :—

	Effect.
Copper,	6
Silver,	6
Gold,	9
Zinc,	18
Platinum,	30
Iron,	30
Tin,	36
Lead,	72
Brass,	18
Gold 1 part, Copper 1,	20
Gold 3, Copper 1,	25
Gold 1, Copper 3,	15
Copper 1, Silver 1,	6
Copper 1, Silver 3,	6

	Effect.
Copper 3, Silver 1,	6
Gold 1, Silver 1,	20
Gold 1, Silver 3,	15
Gold 3, Silver 1,	25
Tin 1, Lead 1,	54
Tin 3, Lead 1,	45
Tin 1, Lead 3,	63
Tin 1, Zinc 1,	27
Tin 3, Zinc 1,	32
Copper 8, Tin 1,	18

If we consider the conducting power of these metals as the inverse of these numbers, it will be as in the following table :—

Table of conducting powers of metals.

	Conducting power.
Copper,	12
Silver,	12
Copper 1 part, silver 1 part,	12
Copper 1, silver 3,	12

* Phil. Trans. 1827, p. 18.

	Conducting power.
Copper 3, silver 1,	12
Gold,	8
Gold 1, copper 3,	4·8
Gold 1, silver 2,	4·8
Zinc,	4
Brass,	4
Copper 8, tin 1,	4
Gold 1, copper 1,	3·6
Gold 1, silver 1,	3·6
Gold 3, copper 1,	2·88
Gold 3, silver 1,	2·88
Tin 1, zinc 1,	2·$\frac{2}{3}$
Platinum,	2·4
Iron,	2·4
Tin 3, zinc 1,	2·25
Tin,	2
Tin 3, lead 1,	1·6
Tin 1, lead 1,	1$\frac{1}{3}$
Tin 1, lead 3,	1·1428
Lead,	1

Probably in order to render these numbers correct representatives of the conducting power of electricity, they ought to be divided by the conducting power of the metals for heat.

The heat produced by a powerful Voltaic battery, when in full action, is very great. Pieces of charcoal are raised to a white heat, even when surrounded with hydrogen or azotic gas, and the light emitted is so intense, that the eye cannot bear the glare. The galvanic light is precisely similar to that of the sun. Objects viewed with it, have precisely the same colours, as when viewed by the light of day.

Mr Children's battery fused and reduced oxides of tungsten and molybdenum. Columbic acid, oxide of uranium, oxide of titanium, and oxide of cerium, were fused but not reduced. Iridium weighing 7·1 grains, was fused into a porous globule whose specific gravity was 18·68.* Spinel, gadolinite, and

* Large plates have a great advantage over a battery composed of numerous small plates, when the object is the production of heat; because the intensity of the electricity being much smaller, the loss is much less.

Part II. zircon were fused, magnesia was agglutinated; but ruby, sapphire, and quartz, were not affected. Diamond powder having been put into the middle of some soft iron wire, it was put into the current, and kept of a dull red heat for six minutes. The diamond powder had disappeared; the interior of the iron was fused into numerous cavities, and all that part of it which had been in contact with the diamond was converted into blistered steel.*

Solid non-conductors becoming conductors when fused. Mr Faraday has shown that *ice* and many other solid bodies are non-conductors while solid, but become conductors when rendered fluid.† The following table exhibits a list of the solid bodies which he examined, and found subject to this law:—‡

1. Ice.

2. Potash, protoxide of lead, glass of antimony, protoxide of antimony, oxide of bismuth.

3. Chlorides of potassium, sodium, barium, strontian, calcium, magnesium, manganese, protochlorides of copper and tin, chlorides of lead, antimony, silver.

4. Iodides of potassium, zinc, lead; protiodide of tin; periodide of mercury.

5. Fluoride of potassium.

6. Cyanodide of potassium; sulpho-cyanodide of potassium.

7. Chlorate of potash; nitrates of potash, soda, barytes, strontian, lead, copper, silver; sulphates of soda and lead; protosulphate of mercury; phosphates of potash, soda, lead, copper; quaterphosphate of lime; carbonates of potash and soda; borax, borate of lead, perborate of tin; chromate of potash, bichromate of potash, chromate of lead; acetate of potash.

8. Sulphurets of antimony and potash.

9. Silicate of potash; chameleon mineral. This law, however, does not hold in every case. Mr Faraday found various

When the 2000 pairs of plates battery belonging to the Royal Institution was in action, it was impossible to walk across the room without being sensible of pretty strong shocks.

* Phil. Trans. 1815, p. 370. † Ibid. 1833, p. 507. ‡ Ibid. p. 512.

bodies which were non-conductors while cold, and which continued non-conductors after they were rendered liquid by heat. The following is a list of these bodies:—* Chap. VI.

Solid non-conductors continuing so when fused.

1. Sulphur, phosphorus, iodide of sulphur, periodide of tin, orpiment, realgar, crystallized acetic acid, mixed margaric acid, and oleic acid, artificial camphor, caffein, sugar, adipocire, stearin of cocoa-nut oil, spermaceti, camphor, naphthalin, rosin, sandarach, shell lac.

2. Perchloride of tin, chloride of arsenic and its hydrate, though liquid, have no sensible conducting power.

3. Boracic acid, green bottle glass; but flint glass in a liquid state conducted a little.

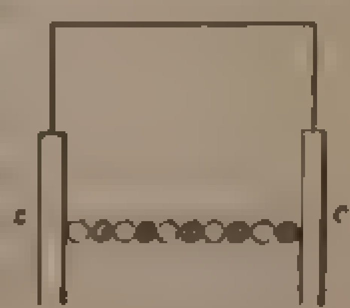
It appears from the experiments of Mr Snow Harris, that heat diminishes the conducting power of metals.†

Dr Ritchie has shown that all compound bodies, whose constituents go to the same pole when decomposed by Voltaic electricity, are non-conductors: hence the reason why oils, resins, camphor, caoutchouc, are non-conductors even when in a melted state. Liquid sulphurous acid is a good conductor, because its constituents, oxygen and sulphur, go to opposite poles. Liquid ammonia is doubtful; because it is uncertain to which pole the azote goes.‡

This law of Dr Ritchie explains the reason why liquid chlorine, sulphur, and phosphorus, are non-conductors; but it does not account for the conducting power of metals both when solid and liquid.

Ritchie's explanation of conduction.

Dr Ritchie conceives that conducting bodies arrange themselves in a peculiar way in order to allow the transmission of electricity through them. Let us suppose a zinc plate and copper plate united together by a wire, as in the margin, to be plunged into water acidulated with sulphuric acid. The zinc plate having a greater affinity for oxygen than the copper wire, will attract the particle of water in its neighbourhood, so that the oxygen extremity of it will place



* Phil. Trans. 1833, p. 513. † Ibid. 1834, p. 231. ‡ Ibid. 1832, p. 279.

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itself in contact with the zinc, and of course the hydrogen side at the greatest distance from it. Let the white circles in the figure represent the atoms of oxygen, and the dark circles the atoms of hydrogen, of which the water is composed. The row of particles between the zinc and copper plates will arrange themselves as in the figure. And unless the constituents of a compound body be such, that they can arrange themselves in this way, the body cannot conduct electricity.

Mr Faraday has adopted the same view; and if I understand him rightly, he has extended it also to the metals. He conceives the ultimate particles, or atoms of the metals, to consist of two poles, one positive and the other negative. The negative pole of the particle nearest the zinc end of the pile is turned towards the zinc, and its positive pole as far off from the zinc end as possible. Every particle arranges itself in the same way, till the last particle turns its positive pole to the copper end. And unless the particles of bodies be capable of arranging themselves in this way, they cannot be conductors.

Were we to adopt this ingenious view of Dr Ritchie and Mr Faraday, it would go far to destroy the notion at present prevalent, that electricity is a fluid or fluids. It would rather seem that it is a force residing at the poles of material atoms.

Faraday's conclusions.

Faraday has drawn the following conclusions from his experiments:—

1. All bodies conduct electricity in the same way from metals to lac and gases, but in very different degrees.

2. Conducting power is in some bodies powerfully increased by heat, and in others diminished, yet without our perceiving any accompanying essential electrical difference either in the bodies, or in the changes occasioned by the electricity conducted.

3. A numerous class of bodies, insulating electricity of low intensity when solid, conduct it very freely when fluid, and are then decomposed by it.

4. But there are many fluid bodies which do not sensibly conduct electricity of this low intensity; there are some which conduct it without being decomposed; nor is fluidity essential to decomposition.

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5. There is but one body yet discovered, the periodide of mercury, which, insulating a Voltaic current when solid and conducting it when fluid, is not decomposed in the latter case.

6. There is no strict electrical distinction of conduction, which can yet be drawn between bodies supposed to be elementary, and those known to be compound.*

Alcohol decomposed by electricity.

I think it rather doubtful whether the following remarkable experiment, for which we are indebted to Dr Ritchie, can be completely reconciled to these views. Dr Ritchie placed alcohol in a Voltaic current, and found it to undergo decomposition. Olefiant gas was given off at the negative pole, and he inferred that water was given off at the positive pole.† Now, as alcohol is resolvable into olefiant gas and water, it can scarcely be doubted that if olefiant gas was given off at the one pole, water must have been given off at the other; if so, the water must have acted the part of an acid, and the olefiant gas that of a base.

Dr Ritchie has shown the inaccuracy of the supposed law that the conducting power of a wire in the Voltaic battery is directly as its diameter, and inversely as the square of its length.‡ Davy first stated that the conducting power of a metallic wire was inversely as its length, and directly as its section, and this law has been confirmed by the subsequent investigations of Pouillet and Ohm.§ Many interesting experiments on the subject have been made by Mr Snow Harris and Mr Christie.

Marianini, professor of *Physique* at Venice, has made a set of experiments to determine the relative conductibility of solutions of various salts, alkalies, and acids in water. I consider it as worth while to state his results as they are the best we have on the subject.‖ I presume the solutions are supposed to be one part of salt in 100 water, though that is not stated:—

* Phil. Trans. 1833, p. 521. † Ibid. 1832, p. 285. ‡ Ibid. 1833, p. 313.
§ Becquerel Traité de l'Electricité, v. 255. ‖ Ibid. iii. 94.

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Conductibility of saline solutions.

Ratios of the conductibility of different solutions to that of water taken as unity.

(The conductibility of sea water is 100.)

Substances dissolved in 100 parts water.	Conducting power.
Hydrocyanate of soda . .	10·96
Hydrocyanic acid . . .	18·27
Liquid ammonia . . .	26·45
Soda	32·06
Phosphate of potash . .	44·74
Borax	43·31
Phosphate of soda . . .	46·00
Tartar emetic . . .	50·07
Sulphate of zinc . . .	51·64
Chlorate of barytes . .	53·23
Potash	55·68
Protochloride of iron . .	56·53
Nitrate of lime . . .	57·00
Acetate of potash . . .	59·02
Nitrate of barytes . . .	60·00
Protosulphate of iron . .	62·26
Bitartrate of potash . .	62·04
Sulphate of magnesia . .	62·64
Acetate of soda . . .	64·09
Bicarbonate of potash . .	66·07
Chlorate of potash . . .	68·09
Carbonate of soda . . .	69·02
Benzoic acid	70·67
Mellitate of ammonia . .	71·15
Sulphate of soda . . .	74·02
Benzoate of potash . . .	76·56
Nitrate of potash . . .	78·03
Sulphate of potash . . .	80·00
Chloride of sodium . . .	84·79
Potash alum	85·00
Citric acid	85·71
Acetic acid	87·00
Tartrate of potash . . .	92·00
Tartaric acid	98·66

Substances dissolved in 100 parts water.	Conducting power.
Chloride of calcium	110·00
Phosphoric acid	127·00
Ferruginous sal ammoniac	136·00
Oxalate of potash	149·00
Sal ammoniac	150·00
Acetate of copper	154·00
Muriatic acid	164·00
Oxalic acid	179·00
Sulphuric acid	239·00
Sulphate of copper	258·00
Nitrate of copper	278·00
Nitrate of silver	298·00
Chloride of gold	307·00
Nitric acid	358·00
Chloride of platinum	418·00

From the preceding table it appears that the acids have the greatest conducting power, while alkaline and saline solutions are in general much inferior as conductors. The conducting power increases with the quantity of salt dissolved, but more slowly when we approach the point of saturation. The conducting power of sulphate of magnesia dissolved in 100 times its weight of water, is to that of the same salt dissolved in 1000 times its weight as 24 to 13.

CHAPTER VII.

OF INDUCTION.

It has been already noticed, in the chapter where the general principles of electricity are shortly stated, that if a neutral body be made to approach an insulated charged conductor, its electricity undergoes a new arrangement. Suppose the insulated conductor to be charged positively, the end of the neutral body next it becomes *negative*, and the end most

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remote *positive*; so that this neutral body becomes excited merely in consequence of its proximity to the charged conductor. This excited state has been called electricity *by induction*. It will be necessary here to enter somewhat more into particulars.

Induction what.

Suppose we have an insulated copper cylinder terminated by two hemispheres, and that we attach to it, from distance to distance, electrometers consisting of two straws, or two elder pith balls, hanging in contact. Suppose we now bring into its neighbourhood another similar conductor charged with electricity, and furnished with similar electrometers, we shall find that the pith balls in the charged conductors which stood at a distance from each other, will approach more and more the nearer we bring it to the neutral body; while in the neutral cylinder the balls diverge more and more, with this remarkable difference, that the divergence diminishes more and more as we approach nearer the middle of the cylinder, which will be found *neutral*, or incapable of affecting the electrometer at all. Suppose the excited body to be *positive*, then the end of the other cylinder next it will be *negative*, the end most remote *positive*, and the centre *neutral*. We shall find also, if we examine the excited cylinder while thus placed, that the end nearest the other cylinder will be positive, and the end most remote negative. The excited cylinder loses none of its electricity, as may be ascertained by examining it before and after the experiment. It follows from this important fact,

Explained.

1. That an excited body exercises at a distance, upon another previously neutral body, an effect which *dissimulates* its own electricity, and renders the other body electric.

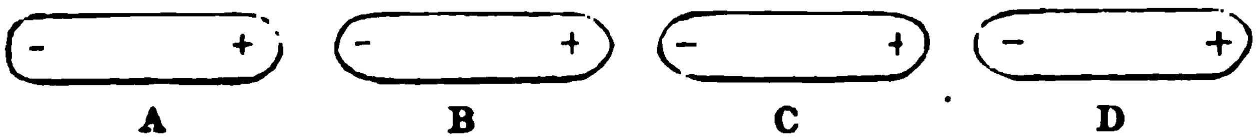
2. That the two electricities are separated in the newly excited body, the electricity opposite to that in the conductor first excited being accumulated in the end nearest to it, and the other electricity in the end farthest distant.

3. That the electricity in the originally excited cylinder is likewise accumulated at the two extremities, the positive at the one end, and the negative at the other. Suppose this cylinder to have been charged with positive electricity: if,

while the other cylinder is near it, we touch the positive end with a finger, we carry off the positive electricity: if we withdraw the other cylinder, we shall find the originally excited cylinder now charged with negative electricity.

We see from this that every body contains the two electricities; but that they do not become sensible till they are separated from each other.

It is obvious that electricity by induction may be induced at great distances. Suppose the insulated cylinder A charged



with positive electricity, the end of B next it will become negative, and the most remote end positive. Let a third neutral cylinder C be placed near B, it will become electric by induction, the end next B being negative, and the most remote end positive. The cylinder D, placed near C, will in like manner become charged with negative and positive electricity, and the same thing will happen however great the number of cylinders thus placed may be supposed to be. We may conceive from this how electricity is transmitted through bodies with inconceivable velocity, without being under the necessity of admitting the actual transmission of one or two electric fluids.

Precisely similar effects are produced when two cylinders, both excited, are brought near each other. Suppose both charged positively, the positive electricity in each will repel the positive and attract the negative; so that the extremities of the cylinders next each other will be, one negative and the other positive; and the same will be the case with the extremities most remote from each other.

Coulomb has shown that the two electricities thus separated by induction are equal in quantity.

Production of electrical currents.

The first attempt to produce electrical currents by the influence of other currents was made by M. Ampere. He formed a spiral with a copper wire covered with silk thread, and passed through an opening in its upper part a silk thread, to which was suspended a copper disc, which was kept for

Part II. some time in the direction of the circumrevolutions of the wire. Having passed the discharge of a strong Voltaic pile through the spiral, he remarked that when the disc was placed between the opposite poles of a horse-shoe magnet, it was sensibly acted on, and the action varied according to the position of the poles of the magnet. This subject has been investigated with much sagacity by Mr Faraday. I shall state the principal facts which he discovered, after giving his new views respecting induction.

Mr Faraday considers induction to be a certain polarity into which the particles of a body are brought—one end being positive, and the other negative—and the state always proceeds from the excited body;* precisely similar to the state of the cylinders electrified by induction, as explained in a preceding paragraph of this chapter.

Mr Faraday found, by a very ingenious set of experiments, that when inductive action was made to pass through different gases, the amount was always the same; so that the inductive action through all gases is the same; and no change takes place whether the gases be rarefied or condensed. The gases tried and compared with each other were the following:—

Azote	Oxygen	Hydrogen
Carbonic acid	Olefiant gas	Arseniетted hydrogen
Carbonic oxide	Protoxide of azote	Sulphuretted hydrogen
Deutoxide of azote	Sulphurous acid	
Ammonia	Fluo-silicic acid	

If we reckon the inductive power of air unity, that of the other bodies tried by Mr Faraday was as follows:—

Shell lac	2
Flint glass	1·75
Sulphur	2·25
Spermaceti	1·8

Mr Faraday has also given the name of *induction* to the power which electric currents possess of exciting in matter.

* Phil. Trans. 1838, p. 1.

which is in the sphere of their activity, any peculiar state which Chap. VII
produces other currents. To show this kind of induction, he rolled into a spiral upon a cylinder of wood, two similar copper wires covered with silk and two or three hundred feet long. One of the spirals was made to communicate with a *multiplicator*, and the other with a Voltaic battery of 100 pairs of four inch plates, the zinc plates being surrounded with copper in Wollaston's manner. The needle immediately deviated a little; but the deviation became very soon insensible. Another deviation took place in the contrary direction whenever the contact with the battery was interrupted. It would appear from this, that these inductions take place only at the instant that the current begins or ceases. From this Faraday concludes, that this momentary current participates more of the current produced by the discharge of the Leyden phial, than that produced by the Voltaic battery. He found that the current induced when the circuit is completed, flows in the same direction as the principal current; but the current which appears when the circuit is interrupted, flows in the opposite direction.*

Mr Snow Harris has shown, that a body electrified by induction is the same in vacuo as in air. So that the tendency of electricity to escape in such a case does not exist.†

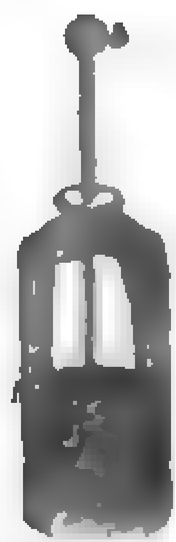
Mr Jenkins observed, that if an ordinary wire of a short length be used as the medium of communication between the two plates of an electrometer, consisting of a single pair of metals, no management will enable the experimenter to obtain an electric shock from this wire; but if the wire which surrounds an electro-magnet be used, a shock is felt each time that the contact with the electrometer is broken, provided the ends of the wire be grasped in each hand.‡ This fact induced Mr Faraday to study the subject. He showed that the effect depended upon the length of the wire, and finally traced the phenomena to the influence by induction of an electric current upon itself.§

* Phil. Trans. 1822, p. 126 † Ibid. 1834, p. 237.
‡ Ibid. 1835, p. 41. § Ibid.

CHAPTER VIII.

OF THE THEORY OF THE LEYDEN JAR, AND OTHER SIMILAR APPARATUS.

It has been already stated in a preceding chapter, that the Leyden jar was accidentally discovered at Leyden, by Cunæus, Muschenbroeck, and Allemand, at a very early period of electrical science. It constituted a memorable era in the history of electricity, because it furnished electricians with a method of accumulating electricity at pleasure, and thus of producing many curious phenomena, and investigating the laws of electricity with greater certainty and success. The theory of the Leyden jar was first investigated by Dr Franklin, and has been long familiar to electricians. The *condenser* and *electrophorus*, instruments so useful in many investigations, are nothing else than modifications of it. The Leyden jar, as at present employed by electricians, consists of a thin cylindrical glass of any size, from that of an eight ounce phial, to that of 400 cubic inches or even more, usually terminated at the upper extremity by a mouth as wide as will admit the insertion of the hand to coat the inside of the jar with tinfoil.



The jar is covered both inside and out with tinfoil, pasted on from the bottom to within a third of the top. The upper portion of the jar is coated over with lac varnish, or if not varnished it should be rubbed over with a thin coating of tallow to prevent moisture from adhering to the glass, which in damp weather or in a moist atmosphere, would prevent the possibility of charging the jar. A cork covered over with lac varnish, or with sealing-wax, is fitted into the mouth of the jar, through which passes a brass wire, terminating at its upper extremity in a knob or small sphere of brass, and dividing within the jar

into three wires, which spread wide, and in consequence press lightly against the inside coating of the jar.

Mode of charging the Leyden phial.

Let us suppose that the knob *b* is placed within the striking distance of the prime conductor of an electrical machine, while the outside coating, in consequence of the jar standing on a common table, communicates with the earth. When the machine is moved, a spark of positive electricity strikes the knob *b*, and makes its way along the wire to the inside of the jar. This spark, from the nature of glass, cannot pass through the jar, but is deposited upon the inside surface of the jar. It will repel the positive electricity on the outside surface of the jar, which will be partially driven out of it, and will pass into the earth; while at the same time it will attract the negative electricity, which will therefore accumulate on the outside surface of the glass. Suppose another spark to pass from the prime conductor to the knob *b*. This will occasion an additional quantity of positive electricity to accumulate on the inside surface of the jar. More positive electricity will be driven out of the outside surface and more negative electricity will be attracted into it. Thus every spark which passes from the conductor to the knob, occasions an increase of positive electricity on the inside surface, and an increase of negative electricity upon the outside surface; till at last there is a very considerable accumulation of the two electricities on the two surfaces. The mutual attraction which takes place between these two electricities, will have a tendency to prevent them from escaping spontaneously, and will also occasion a much greater accumulation of each on their respective surfaces. Suppose now that a good conductor, a metallic wire for example, is made to touch at once the knob *b*, and the outside coating of the jar, the two electricities will rush into combination along this wire, a snap or an explosion will take place accompanied by a brilliant light, the two electricities will combine, and all symptoms both of positive and negative electricity will disappear, or almost disappear. If instead of a metallic wire the medium of communication between the inside and outside coating, be the human body; as for example, if we grasp the jar by the bottom with one hand, and touch

Part II.

the knob with the finger of the other, the two electricities unite in our body, and the act of union is attended with a shock more or less violent and painful, according to the size of the jar, and the greatness of the charge. When a number of jars are united together, constituting what is called an electrical battery, the shock is so great as to prove fatal when passed through the body of an animal of considerable size, as a sheep, or a large dog.

Charge on the surface of the glass.

In the Leyden phial the electricities are not lodged upon the coatings of the jar, but upon the two surfaces of the jar itself. This was demonstrated by Dr Franklin. Instead of coating the inside of the jar with tinfoil, we may fill it with water up to the usual height of the coating. This, indeed, was the coating originally used by the discoverers of the Leyden jar. Instead of pasting the outside coating to the jar, we may make it merely to fit the jar, and so that the jar may be easily slipped out of it without using force. Dr Franklin charged a jar containing water, and having the outside coating merely in contact. He then poured out the water, and drew the jar out of its outside coating, taking care never to establish any communication between the inside and outside. New water was now poured into the jar, and a new coating applied on the outside. The jar was still found charged, and when a communication was established between the knob and the outside coating, an explosion took place just as would have happened had the original coatings not been displaced. Whenever the quantity of positive electricity accumulated on the inside surface of the jar is so great, that by its repulsive force it prevents the accumulation of any more, then the jar has received all the charge which it can take.

If we insulate a Leyden jar, and place the outside coating in contact with an insulated electrometer consisting of two pith balls suspended by linen threads from a hooked wire: as soon as a spark of positive electricity passes into the knob, the balls separate, indicating a charge of positive electricity. If we touch the wire to which the pith balls are attached, they immediately close, because by this contact, we carry off the positive electricity which had been driven out of the outside

surface by the spark of positive electricity, which was deposited on the inside surface. If we now touch the knob of the jar, so as to draw off the uncombined positive electricity from the inside surface, the pith balls again separate. But they are now charged with negative electricity. Thus we see, that when positive electricity accumulates on the inside surface, positive electricity is driven off from the outside surface, and that negative electricity accumulates on that surface. When the jar is charged, if we put it under the receiver of an air-pump, and exhaust the air, the two electricities being no longer retained in their place, rush towards each other, forming a path which appears luminous in the dark.

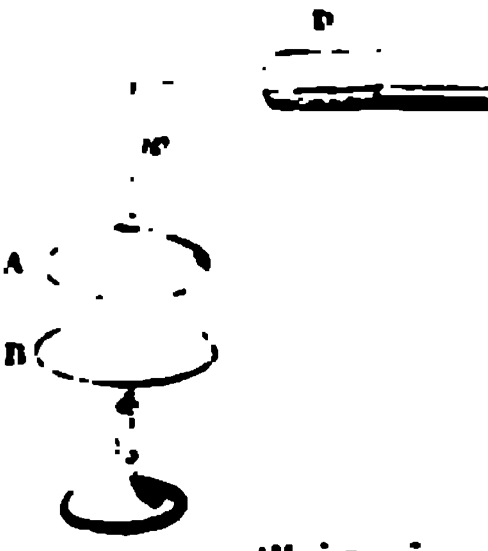
Leyden jar furnishes either electricity.

By means of the charged Leyden jar, we can procure at pleasure either positive or negative electricity, which is very convenient for many purposes. From the mode of charging the phial, the inside has generally a charge of positive electricity, while negative electricity is accumulated on the outside. Suppose it thus charged, if we take it up in one hand, and make the knob rub against the surface of a cake of wax or a thin plate of glass covered with lac varnish; the parts of the plate touched by the knob will have acquired positive electricity, and the parts opposite on the under surface of the glass will have acquired negative electricity. If we now suspend the glass plate insulated in the air, and blow upon it a mixture of sulphur and minium powder previously triturated together, the minium alone will adhere to the parts of the plate that have been electrified positively. If we blow the same mixture of powder upon the other side of the plate, the sulphur alone will adhere to the parts which are electrified negatively. Thus we will have the same lines traced on the two sides of the glass plate; but with different coloured powders. The positive electricity will be covered with the red powder of minium, the negative electricity with the yellow powder of sulphur. This curious experiment was first made by Lichtenberg, and these lines are called the lines of Lichtenberg. The reason of the phenomenon is, that when sulphur and minium are triturated together, sulphur acquires positive electricity, while minium acquires negative: hence they naturally attach themselves

to those surfaces which have an electricity opposite to their own.

Condenser explained.

The instrument called the *condenser*, so useful in electrical investigations, is very nearly of the same nature with the Leyden jar, or at least depends upon the same electrical laws. This instrument was first contrived by Æpinus, but it was brought into the convenient form for use, which it has at present, by Volta. Suppose a body A, to be placed in contact with the charged prime conductor of an electrical machine. It will acquire a certain electrical charge. If while thus charged, we bring the body B, (supposed in a natural state and communicating freely with the ground,) into its neighbourhood, the presence of this new body will enable A to take a much stronger charge than before. For the electricity with which it is charged acts upon the electricity of B. It drives the electricity of the same kind with itself into the ground, and attracts the opposite electricity, and fixes it upon that surface of B which is nearest A. By this attraction, the elasticity of the electricity in A is diminished. The consequence is, that an additional quantity of electricity passes from the prime conductor into A. This new quantity acts in its turn upon the electricity in B, drives an additional portion of the electricity of the same name into the earth, and attracts an additional quantity of the opposite electricity. This new charge of electricity in B, by its attraction, serves still farther to diminish the elasticity of the electricity in A, and thus an additional quantity will flow into it from the prime conductor. Thus the two bodies act reciprocally upon each other, and mutually increase the charge in each.



Let D be the prime conductor of a machine, and let A be a metallic plate suspended from the conductor by means of the wire *w*. If D be weakly charged, it will communicate a weak charge also to A. This may be ascertained by bringing an electrometer into its neighbourhood. Now let the metallic plate B, communicating with the ground, be made gradually to approach A, till it is

as near as possible, without however being sufficiently so to receive a spark from A. The presence of the plate B will enable A to take a much greater charge, as may be easily ascertained by means of an electrometer. If we now remove A by means of a glass rod, taking care that B shall retain its position till A is insulated, then A will be much more strongly charged than it would have been had not B been near it.

The nearer we can bring B to A, the greater is the effect which it will produce in increasing the charge. But this diminution of distance is limited by the tendency which the electricity of A has to pass off to B. The best way then is to interpose between them some non-conductor, through which of course the electricity cannot pass; such for example as a thin plate of glass, or a thin coating of lac varnish. By this contrivance, the distance between the plates may be rendered very small. Thus arranged, the two plates constitute what is called the *condenser*.

Bennet's doubler.

Bennet's *doubler* may be considered as a still farther improvement of the condenser. The first description of it was published in the *Philosophical Transactions* for 1787. The method of using this doubler is as follows:—

It consists of three parts. 1. A polished brass plate A, with an insulating handle fixed in its centre. 2. A similar plate B, with an insulating handle fixed in its periphery. 3. The cap of Bennet's gold leaf electrometer, G, which serves for the third plate. The two plates A and B, are varnished on their under side. The handles are made of mahogany fixed to the plates by means of glass nuts covered with sealing-wax.

Suppose we have to examine the electricity of the plate G.

1. Place B upon G, and communicate some electricity to

Part II. the latter, while the plate B is touched with the finger. It is evident that G will receive a greater charge than if B were not near it. In fact these two plates constitute the common *condenser*.

2. Remove the source of electricity from the contact of G, and take the finger from off B. Raise B, by its insulating handle, and B and G will exhibit the opposite states of electricity more strongly than when they were in contact.

3. Place A on B and touch A with the finger. It is obvious that a quantity of the opposite electricity from that in B will accumulate in A, while the same kind of electricity will be driven off. In short, A will be charged with the same kind of electricity as G.

4. Place B upon G, and touch B with the finger as before, and at the same time apply A edgeways to G. In this situation A will communicate the greatest part of its electricity to G.

5. Remove A, take the finger from B, and raise B from G. The opposite electricities in B and G will now be stronger than before, on account of the additional electricity afforded by A.

6. Place A upon B again, and proceed as in No. 3. An additional dose of electricity may now be communicated from A to G. By proceeding in this manner for a certain time, the electricity originally communicated to G, though at first too small to affect the strips of gold leaf, will at last become sufficiently sensible to produce a considerable divergence in them.

This doubler was still farther improved by Mr Nicholson, who constructed what is called his *revolving doubler*, an account of which may be seen in the *Philosophical Transactions* for 1788, and in *Nicholson's Journal* for May, 1800.

The best condenser consists of two metallic plates, having their contiguous surfaces covered with a coat of varnish. Care must be taken to place them upon each other, without rubbing them against each other. For friction would develope electricity in the coating of resin, which would adhere to it and might occasion errors in delicate experiments. To render the instrument convenient, the plate B is fixed upon a solid

metallic foot, and an insulating handle of varnished glass is fixed in the centre of the upper surface of the plate A. A wire terminating in a knob is usually fixed in the plate A, by means of which, the electricity to be examined is conveyed to it, while the finger is in contact with plate B, or its metal stand in order to secure its communication with the ground. When A is to be removed from B, the condenser should be placed upon a table, and A should be lifted up perpendicularly from B, without any friction, otherwise A would be apt to discharge itself upon B. Ch. VIII.

Theory of the condenser.

The condenser evidently depends upon the same principles as the Leyden phial, and indeed its theory is so obvious, that a very few words will be sufficient to explain it. Suppose plate A to be charged with positive electricity, a certain quantity of negative electricity will be attracted and fixed in plate B. This will allow A to take a farther charge, which will accumulate a still additional quantity of negative electricity in B; and so on. If the distance between A and B were nothing, the quantity of positive electricity in the former would be exactly balanced by the quantity of negative electricity in the latter. But there is always a certain distance between A and B: hence, the positive electricity in A will exceed the negative electricity in B, and it will exceed it the more the greater the distance between the two plates is. But in all cases there must be an excess of vitreous electricity in A; so that the two electr cities accumulated in the two plates, will not exactly neutralize each other. The very same thing happens in the Leyden jar, and hence it will be found, that when the communication is made between the two sides of the jar, all electrical symptoms do not vanish. A small charge will still be found in the jar, which will occasion a slight shock, when, holding the discharged jar in one hand, we touch the knob with the other.

In the condenser, the electricity does not reside in the metallic plates, but on the faces of the thin coat of varnish which separates them. This is easily demonstrated by employing a condenser whose plates are separated by a thin disc of glass. After plate A is charged with electricity, place the

Part II. condenser upon an insulating stool. Then remove plate A. On applying the finger to it, only a very small spark will be given, amounting to no more than a very small portion of the electricity. Now, remove the plate of glass by taking it up by one of the corners, and examine plate B. It also will communicate only a very slight spark. We see then that the two electricities were in fact upon the two surfaces of the glass. Thus the condenser differs from the Leyden phial in nothing but the shape.

Electrophorus. The *electrophorus*, an instrument for which we are indebted to Volta, is quite similar in its nature to the condenser, and depends upon the same principles as the Leyden phial. The electrophorus usually consists of three parts. 1. A metallic plate, round, and about six inches in diameter. It should be thin and provided with an insulating stand. 2. A cake of resin of the same dimensions as the metallic plate. This resin is melted, and then cast upon the metallic plate. It should also be thin. We shall call this metallic plate the *sole* of the electrophorus, and the resin we shall call the *cake*. 3. A metallic plate, or a wooden plate quite smooth, all round, and without any projecting points, covered with a coating of tinfoil. It should be of the same dimensions as the other two parts. We shall call it the *cover*. It must be provided with an insulated glass handle like the upper plate of the *condenser*.

Pour the melted cake upon the insulated sole and let it remain till it has become firm and hard. If we now examine the apparatus, we shall find it negatively electrified, and when the finger is applied to any part of it, especially the sole, a spark is produced. If the apparatus be now suffered to remain at rest, the electricity is gradually dissipated and disappears, but it may be again restored by rubbing the resin with a piece of cat's skin with the fur on. By this friction resinous electricity is developed on the cake. If the insulated sole be placed in contact with an electrometer, we shall find the pith balls diverge, and indicate negative electricity. If we touch the sole with our finger, the pith balls fall down, and all signs of electricity disappear. The cake obviously repels the negative electricity from the sole, and attracts the positive electricity: hence

there will be an accumulation of negative electricity in the cake, and of positive electricity in the sole.

If we apply the cover to the cake, lifting it by the insulating handle, and after allowing it to remain a little, lift it off again, and apply it to an electrometer, we shall find no signs of electricity. But if we replace it on the cake, and place an insulated electrometer in communication with it, the pith balls will diverge with negative electricity. If we touch the cover with the finger, and then withdraw it, we shall find it charged with positive electricity. It is obvious that the action of the cake upon the cover is the very same as upon the sole. The negative electricity has been repelled, and the positive electricity attracted. So that both the sole and the cover, if they be made to communicate with the ground, will be found charged with positive electricity.

As the electricity does not soon leave the cake after friction, if it be kept in a dry place the cover may be charged with positive electricity, simply by placing it on the cake, and touching it with the finger and then removing it; and the charge will be sufficient to give sparks, and thus to enable us to apply the electrophorus to a variety of useful purposes. It may continue to act for a month or even longer. But the attraction between the two opposite electricities will gradually overcome the resistance which the resin manifests to the disengagement of its electricity. The positive electricity will gradually make its way, unite with the negative electricity on the surface of the resin, and all the electricity will of course be discharged. When this happens the electrophorus may be again brought into action by rubbing the cake with a piece of cat's skin as before.

The theory and mode of action of the electrophorus is so obvious after the preceding explanation of the Leyden phial and the condenser, that it is quite unnecessary to offer any farther elucidation.

CHAPTER IX.

OF THE ELECTRIC SPARK.

Spark discovered by Dr Hall.

The first person who seems to have perceived the electric light was Otto Guericke; but he has left nothing satisfactory on the subject. Dr Hall, however, in the year 1708, observed that when pieces of amber are rubbed with flannel they give out a great deal of light, with a crackling noise. This light he compared to lightning. He observed the same thing with the diamond, and lac, and sealing-wax, when rubbed. He found that all bodies which are capable of being excited by friction, or *electrics*, have the property of becoming luminous when rubbed. This friction excites electricity and gives them the property of attracting light bodies. Thus Dr Hall recognized that when electrical bodies are excited by friction they emit light.* Mr Hauksbee had indeed, in 1705, observed the light emitted when mercury is agitated in a glass tube in vacuo, and several other similar phenomena. But he was not aware that electricity had any share in producing this light.† Very soon after the discovery of Dr Hall, we find Hauksbee describing the light evolved by the friction of glass, and various other bodies, and it is obvious, from his account of his experiments, that he was aware that this light was connected with electricity.‡

Positive spark a brush.

When a spark is drawn from a body electrified positively it has the form of a pencil of rays, or *brush*, or it forms a cone, the apex of which is in the body from which the spark proceeds. This brush was first described by Mr Gray, and when originally observed it excited great surprise in him and his friends. When the spark is drawn from a body negatively electrified, it has the appearance of a *star*.

* Phil Trans. xxvi. 69. † Ibid. xxiv. 2129. ‡ Ibid. xxvi. [illegible]

Particular attention has been paid to the electric spark by Mr Faraday.* The conditions requisite for its production in its simpler form, are these:—An insulated *dielectric* (or non-conductor) must be interposed between two conducting substances, in opposite states of electricity. Then if the action be increased, either by bringing the conductors nearer each other, or by increasing the charge, a spark appears, and the two forces lose their intensity, a *discharge* having occurred. The usual conductors are metals, and the usual dielectrics are air or glass.

Chap. IX.

Mr Snow Harris found, that when the dielectric through which the spark passes is *air*, the quantity of electricity necessary to produce a discharge, varied exactly as the distance between the balls from the one to the other of which the discharge passed.† He found also that the quantity of electricity varied as the density of the air. If the quantity of air remained the same, while the distance between the balls and the density of the air varied, these last were found in the inverse ratio of each other, the same quantity of electricity passing over twice the distance with air rarefied to one-half.‡ Mr Harris showed also that variation of the temperature of the air produces no variation in the quantity of electricity required to cause a discharge across a given interval.§

Faraday's opinion of the spark.

Mr Faraday conceives, that before a spark appears, the dielectric between the two conducting substances is brought, by induction, to a certain state of tension, each particle having one of its poles turned to the positive, and the other to the negative ball, and that when this tension rises to a certain amount, discharge takes place. This would indicate a motion of electricity from one conducting ball to the other, which is rather inconsistent with the theory of induction which we have given in a preceding chapter. In fact, it is the electric spark and the discharge of the Leyden phial, which constitute the great difficulties in the way of considering electricity as not a fluid but a force.

Nor is it easy to affix a definite idea to the word *tension*,

* Phil Trans. 1838, p. 95. † Ibid. 1834, p. 225.
‡ Ibid. p. 229. § Ibid p. 230.

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so commonly applied to electricity. Thus we say that the quantity of electricity in the charged Leyden phial is small, but its tension great; and, that the quantity of electricity in an acting Voltaic battery is great, but its tension small. We mean, that the former can make its way through a given thickness of a dielectric much more easily than the latter. According to Mr Faraday's notion of conduction, we might conceive that the polar particles of the dielectric were much more completely turned in the inductive direction, in the former than in the latter; but why a greater quantity of electricity should have a less effect than a smaller quantity in the other we cannot understand.

Effect of different gases.

Mr Faraday ascertained by experiment, the distance through which a spark would pass when different kinds of gas were made to surround the two balls from which the sparks proceeded, and the interval between them. The following table shows the result :—

Gases.	Kind of electricity.	Mean length of spark.
Air . . .	Positive	0·695 inch
Air . . .	Negative	0·635
Oxygen . . .	Positive	0·505
Oxygen . . .	Negative	0·510
Azotic . . .	Positive	0·615
Azotic . . .	Negative	0·645
Hydrogen . .	Positive	0·370
Hydrogen . .	Negative	0·275
Carbonic acid .	Positive	0·640
Carbonic acid .	Negative	0·590
Olefiant . . .	Positive	0·750
Olefiant . . .	Negative	0·730
Coal gas . .	Positive	0·490
Coal gas . .	Negative	0·525
Muriatic acid . .	Positive	1·105
Muriatic acid . .	Negative	0·720

But in repeating these experiments there was a considerable diversity in the results: hence one would be disposed to infer that all the circumstances have not been fully appreciated.

The discharge passed from a small brass ball, having a diameter of 0·93 inch, to a brass ball having a diameter of 2·02 inches. Now, it was observed that the variation in the distance at which the sparks passed, was always greater when

the small ball was positive, than when it was negative. The following table shows the amount of the range in each gas, in fractions of an inch :— Chap. IX.

Gases.	Positive.	Negative.
Air	0·19	0·09
Oxygen . . .	0·19	0·02
Azotic . . .	0·13	0·11
Hydrogen . .	0·14	0·05
Carbonic acid .	0·16	0·02
Olefiant . . .	0·22	0·08
Coal gas . .	0·24	0·12
Muriatic acid . .	0·43	0·08

We see from these experiments that gases have very different insulating powers, and, that these powers have nothing to do with the specific gravity of the gas. Muriatic acid gas is by far the best insulator of all those tried, while hydrogen gas is the worst. Nor is air the same as it ought to be, supposing it a mixture of 4 volumes azotic and 1 volume oxygen gas. Mr Faraday conceives that this insulating power depends upon the ease or difficulty with which their particles are made to assume the polar direction by induction—a conjecture certainly very probable.

Appearance of the spark.

The appearances of the sparks when drawn through different gases were particularly described by Van Marum. It will be sufficient if we give here the observations made on the subject by Mr Faraday. In *air* the sparks have an intense light and bluish colour, and often have faint or dark spots in their course, when the quantity of electricity passing is not great. In *azotic* gas they are very beautiful, having the same general appearance as in common air, but having a more intensely blue colour, verging towards purple. In *oxygen* the sparks are whiter than in air or azotic gas, but scarcely so brilliant. In *hydrogen* they have a very fine crimson colour, which passes away as the hydrogen is rarefied. In *carbonic acid* gas the spark is similar to that in air, excepting that it has a little green in it. It is more irregular than in air. In *muriatic acid* the sparks are nearly white, and are bright throughout never presenting those dark intervals that appear in sparks drawn through air, azotic, and some other gases. In *coal* gas

Part II. the spark is sometimes green, sometimes red, and both colours often appear in the same spark.*

"The spark is a discharge or lowering of the polarized inductive state of many dielectric particles, by a particular action of a few of the particles occupying a very small and limited space. Faraday conceives that the few particles where the discharge occurs are not merely pushed apart, but assume a peculiar state, a highly exalted condition for the time; that is to say, have thrown upon them all the surrounding forces in succession, and rising up to proportionate intensity of condition perhaps equal to that of chemically combining atoms, discharge the powers, possibly in the same manner as they do theirs, by some operation at present unknown to us; and so the end of the whole. The ultimate effect is exactly as if a metallic particle had been put into the place of the discharging particle; and it does not seem impossible that the principles of action, in both cases, may hereafter prove to be the same." I have given this explanation of Faraday in his own words, because I do not clearly understand it. I could form a notion of the particles of air being brought, by induction into a state capable of conducting electricity; and the light emitted, and constituting the spark, might be owing to the sudden condensation of the particles of the elastic fluid through which the electricity passes. We could thus see why a spark appears when electricity passes through gases, and not when it passes through wires.

The *brush* is the next form of disruptive discharge after the spark. If an insulated conductor, connected with the positive conductor of an electrical machine have a metal rod 0·3 inch in diameter, projecting from it outwards from the machine, and terminating by a rounded end or a small ball, it will generally give good brushes; or, when the machine is in bad order, if we approach the hand, or a large conducting surface, to increase the induction, we may obtain a brush from the rounded end of the wire. When the brush is obtained by a powerful machine, on a ball about 0·7 inch in diameter, at the end of a

* Phil. Trans. 1838, p. 109.

long brass rod attached to the prime conductor, it constitutes a kind of cone of light. A short conical bright part, or root, appears at the middle part of the ball projecting directly from it, which, at a little distance from the ball, breaks out suddenly into a wide brush of pale ramifications, having a quivering motion, and being accompanied, at the same time, by a low, dull, chattering sound.* Mr Wheatstone has shown that the brush consists of successive intermitting discharges.† If the eye be passed rapidly, by a motion of the eyeball, across the direction of the brush, by first looking steadfastly about 10 or 15 degrees above, and then instantly as much below, the general brush will be resolved into a number of individual brushes, standing in a row upon the line which the eye has passed over; each elementary brush being the result of a single discharge, and the space between them representing both the time that the eye was passing over the space, and that which elapsed between one discharge and another. Chap. IX.

Brush.

The *brush* is in reality a discharge between a bad or a non-conductor, and either a conductor or another non-conductor. Under common circumstances the brush is a discharge between a conductor and the air. Mr Faraday conceives it to take place in the following manner.‡ When the end of an electrified rod projects into the middle of a room, induction takes place between it and the walls of the room, across the dielectric, *air*; and the lines of inductive force accumulate upon the end in greater quantity than elsewhere; or the particles of air at the end of the rod are more highly polarized than those at any other part of the rod. The particles of air situated in sections across these lines of force, are least polarized in sections towards the walls, and most polarized in those nearer the end of the wires: hence it may happen that a particle at the end of the wire is at a tension that will immediately terminate in discharge; whilst in those only a few inches off, the tension is still beneath that point. Suppose the rod charged positively, a particle of air, A, next it being polarized, and having of course its negative force directed towards the rod, and its

* Faraday, Phil. Trans. 1838, p. 110.

† Phil. Trans. 1834, p. 586. ‡ Ibid. 1838, p. 111.

Part II.

positive force outwards; the instant that discharge takes place between the positive force of the particle of the rod opposite the air, and the negative force of the particle of air towards the rod, the whole particle of air becomes positively electrified. And when, the next instant, the discharged part of the rod resumes its positive state, by conduction from the surface of metal behind, it not only acts on the particles beyond it, by throwing A into a polarized state again, but A itself, in consequence of its charged state, exerts a distinct inductive action towards these farther particles, and the tension is consequently so much exalted between A and B (the next particle of air), that discharge takes place there also, as well as again between the metal and A.

The brush has specific characters in different gases, indicating a relation to the particles of these. bodies even in a stronger degree than the spark. Faraday found no difference in the brush, whatever conductors he used from which the brushes originated. He tried wood, card, charcoal, nitre, citric acid, oxalic acid, oxide of lead, chloride of lead, carbonate of potash, melted potash, strong solution of potash, oil of vitriol, sulphur, sulphuret of antimony, and hæmatite, without any variation in the character of the brushes obtained.*

Effect of gases on.

The following are the effects which he observed with different gases at the positively charged surfaces, and with atmospheres varying in their pressure. The general effect of rarefaction was the same in all the gases tried: at first sparks passed; these gradually were converted into brushes, which became larger and more distinct in their ramifications, until upon further rarefaction the latter began to collapse and draw in upon each other till they formed a stream across from conductor to conductor: then a few lateral streams shot out towards the glass of the vessel from the conductors. These became thick, flossy, and soft in appearance, and were succeeded by the full constant *glow*, which covered the discharging wire.

Fine positive brushes are easily obtained in *air* at common pressures, and possess a purple light. When the air is rarefied

* Phil Trans. 1838, p. 117.

the ramifications are very long, filling the globe, the light is generally increased and is of a beautiful purple colour, with an occasional tint of rose red. In *oxygen* the brush is very close and compressed, and of a dull whitish colour. When the gas is rarefied the form and appearance are better, and the colour purplish, but much poorer than in air. *Azotic* gas gives brushes with great facility at the positive surface, far beyond any gas tried by Mr Faraday. When the gas is rarefied the brushes are magnificent. *Hydrogen*, gas at common pressure, gives a better brush than oxygen, but not equal to that in azotic gas. In rarefied hydrogen the ramifications are very fine in form and distinctness, but pale in colour, with a soft, velvety appearance; greatly inferior to those in azotic gas. When the gas is very much rarefied, the colour of the light is a pale grey green. In *coal gas* the brushes are rather difficult to produce; they are short and strong, generally of a greenish colour, and possessing much of the spark character: in rarefied coal gas the forms are better. *Carbonic acid* produces a very poor brush at common pressures, both in size, light, and colour. In rarefied carbonic acid gas the brush is better in form, but weak as to light; being of a dull greenish or purplish hue, varying with the pressure. It is very difficult to obtain a brush in *muriatic acid* gas. On gradually increasing the distance of the balls to about an inch, the discharge was silent and dark: occasionally a very short brush could for a few moments be obtained, but it quickly disappeared again. On rarefying the gas the formation of the brush was facilitated; but it was generally of a low squat form, very poor in light, and very similar on both the positive and negative balls.*

It used to be said by electricians, that a point charged positively gives a *brush* to the air, while a point charged negatively gives a *star;* but Mr Faraday assures us that this is true only of bad conductors, or of metallic conductors charged intermittingly or otherwise controlled by collateral induction. If metallic points project freely into the air, the positive and negative light on them differ little in appearance, and the

* Faraday, Phil. Trans. 1838, p. 118.

Part II.

difference can be observed only on close examination. If a metallic, wire with a rounded termination in free air, be used to produce the brushy discharge, the brushes obtained when the wire is charged *negatively* are very poor and small compared with those obtained when the wire is charged *positively*. If a large metal ball connected with the electric machine be charged positively, and a fine uninsulated point be gradually brought towards it, a star appears on the point when at a considerable distance; which, though it becomes brighter, does not lose its star form till it is close up to the ball. But if the ball be charged negatively, the point, at a considerable distance, has a star on it; but when brought nearer, a brush is formed on it extending to the negative ball. When still nearer, the brush ceases and sparks are discharged.*

When sparks are passed between two uninsulated knobs of different sizes, far larger sparks pass when the small ball is positive and the large one negative, than when they are in the opposite states of electricity.†

The glow.

What Mr Faraday calls the *glow*, is a clear phosphorescent light, extending a very little way from the metallic point in air. Increased power in the machine tends to produce the glow; for rounded terminations which only give brushes when the machine is in weak action, will readily give the glow when it is in good order. Diminution of the charging surface will produce it. Thus Mr Faraday found that when a rod 0·3 inch in diameter with a rounded termination, was rendered positive by free air, it gave fine brushes, but sometimes also a glow. With a wire of 0·2 inch diameter, the glow was more readily produced; and still more readily when the diameter of the wire was still farther reduced. Its production is very much facilitated by rarefying the air.

To obtain a negative glow at common pressures is difficult. A current of wind usually proceeds from or to the glow; if this be interfered with, the glow disappears. The glow seems to depend on a quick and almost continuous charging of the air close to or in contact with the conductor. When electricity

* Faraday, Phil. Trans. 1838, p. 120. † Ibid. p. 125.

is discharged in rarefied air there is an extended glow from the negative knob, and a dark interval at the positive termination.*

Mr Snow Harris has shown that the distance to which a spark can fly is proportional to the quantity of electricity accumulated.† This distance increases directly as the density of the air diminishes. Mr Harris has shown also that the spark passes through just the same interval when air is heated, without altering its bulk, as it does when the air is cold: hence heated air is not a conductor of electricity, and heat only lengthens the spark by rarefying the air.‡

Electric the same as solar light.

When the electric light is examined by means of a prism, it is found composed of the same seven coloured rays as the solar light. But the proportion of the different rays, and of course the colour of the electric spark, varies according to circumstances.

When a given quantity of electricity occasions a spark by passing from one body to another, its brilliancy is always the greater the smaller the size of the body from which it is drawn: hence it happens that more brilliant sparks may be drawn from a small brass knob, fixed to the prime conductor of an electrical machine, than from the prime conductor itself. A short spark is always white; but a very long spark is usually reddish, or rather purplish. When we draw a spark from the prime conductor of an electrical machine, by means of a metallic knob, the spark is white; but when we draw it by the hand it is purple. If we draw it by means of a wet plant, or water, or ice, the colour is red. The same spark will vary in colour according to its length. When short it is always white, when very long it is purple or violet. A spark which in the open air does not exceed a quarter of an inch in length, will appear to fill the whole of an exhausted receiver, four inches wide, and eight inches long. In the former case it is white, in the latter the light is very feeble, and the colour violet.

Resembles the light of burning bodies.

Thus the electric spark, like the light from burning bodies, does not always contain all the rays which exist in solar light.

* Faraday, Phil. Trans. 1838, pp. 136, 138.

† Phil. Trans. 1834, p. 226. ‡ Ibid. p. 230

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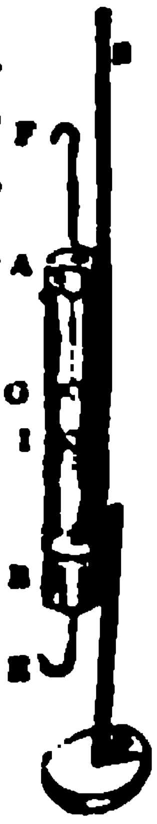
When the spark is feeble, only the most refrangible rays appear; when it is strong all the rays appear together, rendering the light of the most dazzling whiteness. The electrical brush is always violet coloured. When a spark is passed through a torricellian vacuum, made without boiling the mercury, the colour is indigo. Now, this is precisely what takes place in combustion. When the combustion is feeble, the colour of the light is blue; but when it is strong the colour is white.

Probably owing to the condensation of air.

It is well known that electricity occasions a current in the air. This current, indeed, may be felt with the finger, and it has been frequently employed to produce very rapid motions in light bodies. A great variety of amusing exhibitions of this sort are usually shown by lecturers on electricity; most of which may be found described in the popular treatises on electricity, which are in every body's hands. It is evident from this, and from the prodigious rapidity with which electricity moves, that when an electric spark passes through air, that fluid must be suddenly and enormously compressed. Now, it has been shown in the first part of this work, that when air is suddenly compressed sufficiently it gives out light, and that the colour of the light depends upon the rapidity and extent of the compression. This circumstance has led electricians to suspect that the light of the electric spark is not inherent in electricity; but merely the result of the compression of the air.



That air is violently and suddenly compressed when an electric spark is passed through it, may be shown by the following contrivance, for which we are indebted to Mr Kinnersley of Philadelphia:—A B, is a glass tube about ten inches long, and nearly two inches in diameter. It is closed air tight at both ends by two brass caps. Through a hole in the upper cap passes the small glass tube H, open at both ends, and the bottom of it is plunged into a little water in the lower end of the tube A B. Through the middle of each of the brass caps a wire, F G and E I, is introduced, terminating each in a brass

knob within the tube, and capable of sliding through the caps, so as to be placed at any distance from each other at pleasure. If the two knobs be brought into contact, and a Leyden jar be discharged through the wires, the air within the tubes undergoes no change in its volume; but if the knobs G and I be placed at some distance from each other when the Leyden jar is discharged through the wires, a spark passes from the one knob to the other. The consequence is a sudden rarefaction of the air in the tube, shown by the water instantaneously rising to the top of the small tube, and then as suddenly subsiding to H. After which it gradually sinks again to the bottom of the tube as the air slowly recovers its original volume.

Only appears when electricity passes through elastic media.

The most valuable set of experiments which we possess on this interesting branch of electricity are those of Sir Humphrey Davy, published in the *Philosophical Transactions* for 1822. By boiling mercury for some time in a bent barometrical tube, shut at one end, through which a platinum wire passed, he contrived to form a torricellian vacuum free from air. But the vapour of mercury was quite sensible in it. In all cases when the mercurial vacuum was perfect, it was permeable to electricity, and was rendered luminous by either the common spark, or the shock from a Leyden jar, and the coated glass surrounding it became charged. But the degree of the intensity of this phenomenon depended upon the temperature. When the tube was very hot, the electric light appeared in the vapour of a bright green colour, and of great density. As the temperature diminished, it lost its vividness, and when it was artificially cooled to 20° below zero, it was so faint as to require considerable darkness to be perceptible. The charge likewise communicated to the tin or platinum foil was higher, the higher the temperature. This, like the other phenomena, must depend upon the different density of the mercurial vapour.

In all cases, when the minutest quantity of rare air was introduced into the mercurial vacuum, the electric light changed from green to sea-green, and, by increasing the quantity of air, to blue and to purple; and when the tem-

Part II. perature was low, the vacuum became a much better conductor.

A vacuum being made by means of fused tin, the electric light at temperatures below 0° was yellow, and of the palest phosphorescent kind, requiring almost absolute darkness to be perceived, and it was not perceptibly increased by heat.

Davy tried to form a vacuum above boiling olive oil, and above chloride of antimony, which boils at about 388°. The light produced by the electricity passing through the vapour of the chloride, was much more brilliant than that produced by its passing through the vapour of oil—and in vapour of oil it was more brilliant than in vapour of mercury. The light was pure white in the vapour of the chloride, and of a red inclining to purple in the vapour of the oil, and, in both cases, permanent elastic fluid was produced by its transmission.

When the temperature was diminished, the electrical light (transmitted through vapour of mercury) diminished also till the temperature was reduced to 20°. But between 20° and —20° it seemed stationary.

Unless the electrical machine was very active, no light was visible during the transmission of the electricity: but that the electricity passed was evident, from the luminous appearance of the rarefied air in other parts of the syphon. When the machine was in great activity, there was a pale phosphorescent light above, and a spark on the mercury below, and brilliant light in the common vacuum. A Leyden jar, weakly charged, could not be made to transmit its electricity by explosion through the cooled torricellian vacuum: but the electricity was slowly dissipated through it—and when strongly charged, the spark passed through nearly as much space as in common air, and with a light visible in the shade. At all temperatures below 200°, the mercurial vacuum was a much worse conductor than highly rarefied air; and when the tube containing it was included in the exhausted receiver, its temperature being about 50°, the spark passed through a distance six times greater in the Boylean than in the mercurial vacuum.

From these, and various similar experiments related by

Davy, it seems demonstrated that electricity is capable of passing through a perfect vacuum, but that the light emitted, depends upon the vapour or air through which it passes, and that if the vacuum were perfect no light whatever would appear. Chap. IX.

The appearance of the electric spark, when it passes through water, seems to show that the light is visible only when the electricity passes from one body to another, not by conduction, but through elastic media; for the electric spark may be easily made visible in water. But if we add a quantity of acid to the water, it is scarcely possible to make the spark visible in such a mixture. Now, the addition of an acid increases very materially the conducting power of water.

Supposed to be owing to the union of the two electricities.

It was an opinion maintained about thirty years ago, by many eminent experimenters in Germany, that the electric light is of the same nature with *fire*, and that it is formed by the union of the two electricities. This opinion appears to have been first stated by Winterl; and, unless I misunderstand Ritter, he seems to have entertained the same sentiments. But this opinion, though it appears at first sight plausible—and though it would be very convenient to be able to account so well for the analogy which obviously exists between fire and electricity—will not bear a rigid examination.

But erroneously.

Every person who has seen an electric spark, must be aware that the passage is so instantaneous that it is impossible to say from which point it proceeds, or to which it goes. If the spark be long, that is to say if the distance between the two knobs between which it passes be considerable, the presence of the two kinds of electricity may be at once observed. Suppose one of the knobs attached to the prime conductor of an electrical machine, and the other attached to a conducting body connected with the earth—the portion of the spark nearest the prime conductor of the machine exhibits all the characters which distinguish positive electricity—while the portion of the spark nearest the other knob, exhibits the characters of negative electricity. There can be no doubt, therefore, that every spark is composed of the two electricities. When two charged bodies are placed within the striking distance,

Part II. no spark will pass unless the one body be charged with positive, and the other with negative electricity. The two electricities are attracted towards each other, advance at the same instant from each of the charged bodies, and uniting together somewhere between the two knobs, all symptoms of electricity are at an end. When a spark is short, the whole distance between the two knobs through which it passes, is equally illuminated; but when the spark is long, those portions of it which are next the knobs, are much brighter than towards the centre of the spark. Near the knobs the colour is white, but towards the centre of the spark purplish. Indeed, if the spark be very long, the middle part of it is not illuminated at all, or only very slightly. Now, this imperfectly illuminated part is obviously the spot where the two electricities unite, and it is in consequence of this union, that the light is so imperfect.

From all this, it is sufficiently obvious that both the electricities, when in an insulated state, are capable of giving out a brilliant light; consequently the electric spark cannot be owing to the two electricities combining together. Whoever has observed the passage of long sparks from one body to another, cannot but have remarked, that the place where the light is the least, or where there is an interval of no light, lies much nearer the negative than the positive knob. In general, about two-thirds of the spark consist of positive electricity, and about one-third of negative electricity. The cause of this difference between the length of the two electricities in such cases, we have no means of explaining.

CHAPTER X.

OF ELECTRICITY BY FRICTION.

It has been stated in a former chapter, that when two electrical bodies are rubbed against each other, both are excited; the one becoming positively electrified, and the other negatively. The state induced depends upon the bodies rubbed, and a table was given showing the order in which various bodies may be placed so as to indicate which of the two, when rubbed against each other, will become positive, and which negative.* This law may be extended to all bodies, both non-conductors and conductors; only that when we employ conducting bodies, we must take the precaution to insulate them before rubbing them against each other. So slight are the circumstances which occasion the kind of electricity evolved in each body, that it is very difficult to investigate the subject.

Negative electricity evolved by the greatest friction.

If we take two ribbons of white silk, cut from the same piece, and make them rub against each other, while they cross each other at right angles, the piece which crosses the other transversely assumes *negative* electricity, while the other becomes *positive*. In this case, the points of the ribbon that become negative are obviously exposed to a greater degree of friction than those of the other; they are subjected to greater agitation and become more heated. These it would appear are the conditions requisite to induce negative electricity. A similar effect is sometimes produced by rubbing two sticks of sealing-wax, placing them at right angles to each other. The one which crosses the other at right angles experiences the greatest friction, and sometimes rubs off small particles of the other. In this case the two electricities, though disengaged,

* See Chapter I., page 326.

Part II. reunite immediately if the two sticks are left in contact of each other. It seems to follow from these examples, that when two bodies are rubbed against each other, the one which experiences the greatest friction becomes negative, and the other positive.

Hence the reason why, when smooth glass and rough glass are rubbed against each other, the rough glass becomes negative. Its particles must be more agitated and displaced than those of the smooth glass.

Delarive's experiments. M. Delarive found that when we pass the finger, or any substance whatever, over a metallic surface in contact with one of the plates of the condenser, there is a sensible disengagement of electricity. To make this experiment, the hand must be dry. If it be moist, no effect is produced.* The metals rubbed by the hand do not all take the same kind of electricity: antimony, for example, becomes negative, and bismuth positive—the reverse of what happens when the two metals are rubbed against each other.

When we employ as a rubber ivory, horn, cork, or wood, the following metals become negative:—

Rhodium	Tellurium
Platinum	Cobalt
Palladium	Nickel
Gold	

The following metals,

Silver	Brass
Copper	Tin

are also usually negative, but sometimes positive, especially tin.

Antimony is generally strongly negative, though sometimes also it is observed positive. The kind of electricity assumed by iron and zinc is very variable. Lead and bismuth are always positive.

To study the causes of these differences, we must form with different metals, such as antimony, bismuth, and lead,

* Mem. de la Soc. de Physique et d'H. N. de Geneve, vi. 172.

and those which develope the opposite electricity, such as tin, zinc, and iron, cubes having a square inch for each side, in order to operate sometimes on the most or least polished faces and sometimes on the edges. The following are the results obtained by operating in this way:—In very dry air, and when the finger or rubber is dry, the uncertain metals, or those which assume contrary electric states, are always negative, whether the surface rubbed be polished, rough or oxydized. When the surface rubbed is large, the metal becomes positive, if we draw the rubber along its whole extent; and most easily when we rub it with cork.

An augmentation of temperature in one of the two bodies increases the effect, and modifies its nature. Iron, tin, and zinc, exposed immediately to a high temperature, and rubbed on a smooth surface, give most commonly positive electricity, and negative when they are rubbed upon the edges. The greater or less rapidity of the friction has no effect upon the kind of electricity induced.

His explanation.

M. Delarive explains these, and many similar phenomena, from this principle, which he assumes as true, that "all metals when they are rubbed by the hand, cork, wood, &c., become negative provided they be rubbed upon a rough surface." He ascribes the change in the kind of electricity which these metals exhibit, to the property which certain metals have of forming a thin coat of oxide on their surface. This is removed by rubbing, so that the friction no longer takes place between the metal and wood, but between the metal and the coat of oxide which covers the surface of the rubber, the metal being always positive with respect to this oxide. When the coat of oxide is thick and cannot be removed, the friction takes place between the rubber and that oxide. In this case the metal contributes nothing to the kind of electricity induced.

We obtain more readily signs of positive electricity in these metals when we rub them with cork or caoutchouc, because the gentler the friction the more easily is the coat of oxide removed. These results of Delarive show the precautions which ought to be taken in making experiments relative to the disengagement of electricity by friction.

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The state of the surface has such an effect in these experiments that in *cyanite* certain faces become positive when rubbed, and others negative, without it being possible to discover any difference between them.*

Coulomb's experiments.

Coulomb's experiments are calculated to throw some light upon this subject. They were made known to the scientific world by M. Biot, who had in his possession the manuscripts of this celebrated philosopher.† A ribbon of hot paper rubbed against white linen cloth becomes always negative. The same thing happens when it is rubbed against a metal, unless this latter has a great degree of polish, in which case it gives sometimes very feeble indications of positive electricity. When rubbed against white silk cloth, it becomes equally negative; but when both bodies have the same temperature, the paper gives frequently very weak signs of positive electricity. When hot paper is rubbed against black silk, it always assumes positive electricity; but when the black silk is half worn, the hot paper acquires negative electricity.

A hot ribbon of white silk always becomes negative when rubbed against metal; but if we allow it to cool, it gives very weak indications of positive electricity when the metal is finely polished. A black silk ribbon, whether hot or cold, becomes always negative when rubbed against a metal, whether the metal be polished or not. When the ribbon has but little of the silky texture, it sometimes gives signs of positive electricity.

When a piece of silk cloth is agitated with a certain rapidity in the air, it becomes negatively electric: the air of course must become positive.

A ribbon of silk, paper, or linen, rubbed against the skin of an animal still covered with hair, always becomes negative, and the intensity is always greater than when other rubbers are used.

All these examples favour the opinion stated at the beginning of this chapter, that those animal and vegetable tissues whose parts are the most lax, and which therefore experience

* Haüy's Traité de Minéralogie, iii. 226.

† Traité de Physique, ii. 354.

a greater displacement than metallic bodies against which they are rubbed, acquire negative electricity. It would appear also that heat disposes bodies to assume the state of negative electricity. By separating the particles of bodies farther from each other, it brings them under the class of bodies whose surfaces are covered by slight asperities.

Dessaignes' experiments.

The experiments of M. Dessaignes confirm these notions.* He studied the effect produced by immersing bodies in mercury. He distinguished three kinds of immersions, namely, rapid, slow, and leaving the body in the mercury for some time. The substances before their immersion were kept in a glass jar containing quick-lime, that they might be as dry as possible. Glass, sulphur, sealing-wax, and amber, at the temperature of 50°, do not become electric when immersed in either of these modes, nor though the temperature be raised as high as 64½°, provided that of the mercury keep pace with it: amber begins to assume electric properties at 52°, sealing-wax at 59°, and glass at 68°. These bodies must be left in the mercury till the equilibrium of heat be restored; they must then be withdrawn slowly. Between 192° and 212° all electricity disappears, even when the immersion is rapid. According to Dessaignes, none of these four bodies are electric unless the immersion be accompanied by mechanical pressure.

He found that cotton, paper, and linen, became very electric when immersed in any of the three modes between 50° and 192°. Glass, sulphur, amber, and sealing-wax, are always electric when they are a little hotter than the mercury: one or two degrees is sufficient. A cylinder of glass at 212° does not become electric when plunged into mercury at 64°; nor, supposing the glass at 64°, does it become electric when plunged into mercury between 140° and 176°; but it becomes electric when the temperature of the mercury is between 104° and 122°.

With respect to the kind of electricity, M. Dessaignes found that when the mercury in the barometer is rising, and

* Jour. de Phys. lxxiii. 230.

Part II. the temperature of the atmosphere becoming colder, glass, amber, and sealing-wax, cotton, silk, and linen, when plunged into mercury are always negative; but they are positive when the barometer is falling and the atmosphere becoming warmer. Sulphur was always positive. During summer he always found these bodies positive in impure, and negative in pure mercury.

Cold, as well as heat, destroys the electricity in these experiments.

We see in these results, that with respect to amber, sulphur, sealing-wax, and glass bodies, the particles on the surface of which are not easily displaced, simple contact with mercury does not evolve electricity, but that we must produce an agitation on their surfaces; while with cotton, paper, and linen, the particles of which are easily deranged in consequence of their elasticity, a slight agitation is sufficient to produce electric phenomena.

M. Dessaignes tried also what was the influence of heat and cold on metals to modify the state of their electricity when rubbed against other bodies. The metals tried were gold, bismuth, zinc, antimony, and lead. These metals when cooled down to 32°, do not become electric by friction. During the great heats of summer the state of their electricity is quite the reverse of what it is in winter. In summer, heating renders them positive, and they become negative on cooling. The reverse happens in winter.

M. Dessaignes affirms also, that the density of the air has an influence on the electricity of these metals. When the barometer is high and the wind north-east or north-west, the metals are highly electric. In such a case they are always positive when the temperature of the air is increasing, and negative when it is diminishing.

In the present imperfect state of our knowledge, I have thought it right to state all the important facts respecting the evolution of electricity by friction, as they have been collected by M. Becquerel,* though the explanation of them is not very

* Traite de l'Electricite, ii 121.

obvious. Those of Dessaignes in particular seem of difficult explanation. New experiments are still wanting to enable us to construct even a plausible theory. Chap. XI.

CHAPTER XI.

OF ELECTRICITY BY HEAT.

It has been long known that heat produces considerable changes upon the conducting power of various bodies. Some of the best electrics become conductors, when heated sufficiently. Thus *glass* when heated to redness, becomes a conductor.* In like manner *resins* when melted become conductors. Baked wood at the common temperature, is a non-conductor; but when heated very hot, as when just taken out of the oven, it is a conductor. Dr Priestley observed, that a charge could not be passed through melted tallow.† Though there can be no doubt that it conducts much better than cold tallow.

Heat renders bodies conductors.

But much new light has been thrown upon this subject by the experiments of Faraday. In the sixth chapter of this treatise, an abstract of these experiments has been given. He has shown that many bodies which are non-conductors in their solid state become conductors when melted. A table of fifty bodies which are in this predicament has been given in page 376 of this work; while twenty-five bodies are enumerated which are non-conductors both while solid and fluid. Nor can any satisfactory explanation be given of the cause of this remarkable difference; but as these bodies are changed from a solid to a liquid state by the addition of heat, it seems to follow that in many cases at least, heat has a tendency to increase the conductibility of bodies. Faraday conceives that the conductibility is favoured by liquidity, because when bodies are in a liquid state their particles can more easily assume that polar state which he considers as the sole cause of conductibility.

* Priestley; Hist. of Electricity, p. 610. † Ibid. p. 615.

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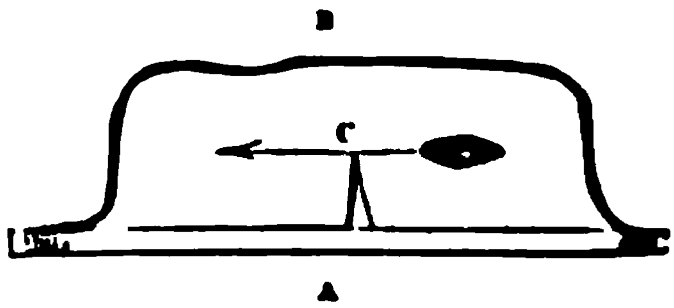
But if this were the only reason, it is not easy to see why every solid body should not become a conductor when liquified.

Electricity evolved by heating two metals.

The magnetic needle is so delicate a test of the presence of an electric current, however small, that it has enabled men of science to discover its existence, where it was not before suspected to exist. Seebeck discovered that if a bar of antimony, A, (see fig. in the margin,) eight inches long, and about an inch wide, have a slip of copper B, or a copper wire soldered, or firmly tied to its two extremities, and



bent into a rectangular shape, if we apply the heat of a lamp to one of the extremities of the antimony bar, a needle placed within the current as at C, will be deflected from the magnetic meridian, and will tend to place itself at right angles to the antimony bar; showing that a current of electricity is passed through the circuit composed of the antimony and copper.* Dobereiner repeated this experiment, the bar A being of antimony, and B of copper, found that the heat of the hand was sufficient, when applied to one of the extremities of A, to cause the needle to deflect ten or twelve degrees from the magnetic meridian.†

Of the metals, bismuth and antimony are the two which produce the greatest effect, when they are used together; bismuth being the most negative, and antimony the most positive of all the metals hitherto tried as thermo-electrics.

Order of the metals.

An important series of experiments was made upon the metals, as thermo-electrics, by Dr Trail of Edinburgh, and Mr Scoresby,‡ and likewise by Professor Cumming of Cambridge.§ He found the order of the metals, as thermo-electrics, beginning with the most negative, and terminating with the most positive, as follows:—

* Annals of Philosophy, (2d Series,) iv. 318. And Gilbert's Annalen der Physik, lxxiii. 430.

† Ibid. p. 115.

‡ Edinburgh Phil. Trans. ix. 465, and Annals of Philosophy, vi. 449.

§ Annals of Philosophy, (2d Series,) v. 427; vi. 177, 288, 322.

1 Galena	Rhodium
2 Bismuth	Gold
3 Mercury }	Copper
4 Nickel }	Ore of iridium and osmium
5 Platinum	Silver
6 Palladium	Zinc
7 Cobalt }	Charcoal }
Manganese }	Plumbago }
8 Tin	Iron
Lead	Arsenic
Brass	Antimony

The place of *tellurium* in the series is immediately above *antimony*.

Every one of the metals in this series gives a *western* declination to the north pole of a magnet placed immediately below it, when it is united with any of those that precede it in the list; and an eastern declination when it is united with any of those that follow it in the series; it being understood that a portion of the circuit is heated by the application of a spirit lamp.

It is obvious that the above order of the metals is very different from that which they follow when placed in the galvanic pile along with liquid conductors. No satisfactory explanation of this difference has yet been given. It is probably connected with the goodness of the metals, as conductors of heat.

Heat not necessary for these effects.

In order to produce thermo-electric effects, it is not necessary to apply heat. Any thing which alters the temperature in one part of the chain, from that of the rest, occasions a deviation in the declination of the magnet: for example, if we produce cold in any part of the antimony bar, by applying ether to it, and allowing it to evaporate; or if we cool it by the application of ice. The greatest effect of all is produced on the magnet, when one part of the bar is heated, and the other cooled. It is evident from this, that the evolution of electricity depends upon the difference in the temperature of different parts of the metallic chain.

Seebeck has even ascertained, that in order to produce

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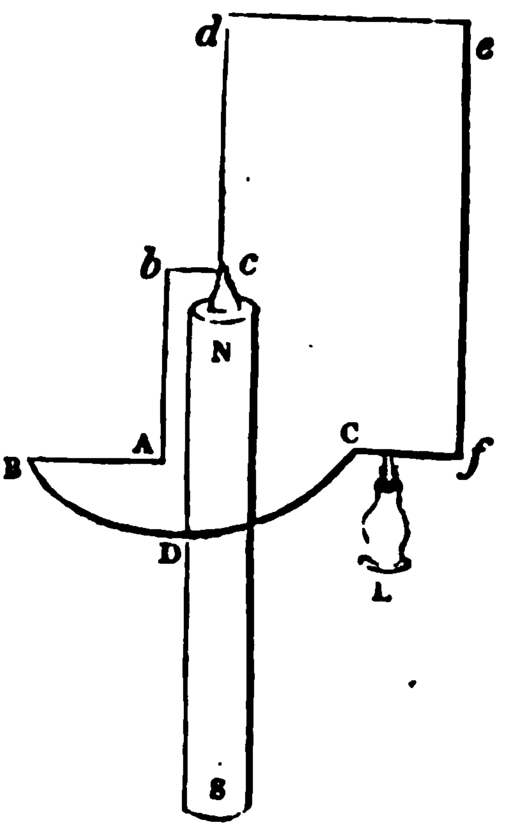
thermo-electric effects, two different metals are not necessary.* A single metal will answer, provided there be differences in its texture and cohesion. Seebeck cast rings of antimony, bismuth, and zinc, and took care that certain parts of them were cooled much more rapidly than others. The parts rapidly cooled assumed a fine grained texture, and a greater degree of density, while the other portions of the ring that were slowly cooled became crystalline in their texture. When these rings were heated at the point of contact of the different textures, they acted upon the magnet. There is a curious experiment of Becquerel, which proves the same thing. He heated to redness, one of the ends of the wire which constitutes the multiplier of Schweigger, and brought it, while in this state, in contact with the other end of the wire which was cold. The needle immediately suffered a declination, showing the evolution of a current of electricity. The thicker the wire of the multiplier is, the more easily does this experiment succeed; probably because the difference of temperature immediately disappears in very small wires. When the two extremities of the multiplier are soldered together, or when heat is applied at a distance from the point of contact, no sensible action is produced on the magnet. But if we touch the wire in the neighbourhood of the heated place, with a thick cold wire of the same metal, the magnet is immediately affected. It would seem from this, that a mere difference of temperature is not sufficient to evolve electricity, but that the heat must also be abstracted more rapidly from one of the sides of the heated point, than from the other side.

But a difference of temperature.

Even liquids may be made to act as thermo-electrics. Seebeck found that concentrated acids—for example, the sulphuric, nitric, and muriatic—occupy a place above bismuth, while the concentrated fixed alkalies stand at the other extremity of the series below antimony. When the acids are diluted with water, they approach the middle of the series. But water produces no effect upon the alkalies. Water and liquid ammonia occupy very nearly the middle of the series.

Thermo-electric rotation.

* Gilbert's Annalen der Physik, lxxiii. 431.



Professor Cumming discovered, that rotatory motions may be induced by thermo-electrics, as well as by common electro-magnetism. The figure in the margin represents the contrivance which he employed. Platinum and silver wires were soldered together, poised upon a magnet and heated by a spirit lamp. A B D C represents the platinum wire, A *b c d e f* C the silver wire. N S is the magnet, *c* N the support of the wires, and L a spirit lamp. The platinum wire is so much thicker than the silver wire, that the part A B balances the projecting part of the silver wire *d e f* C. A wire is attached to *d e* at right angles, with a small weight to counter-balance B D C.* He afterwards found it more convenient to bend the parallelogram into the form of a semicircle. When the lamp and magnet are placed opposite to each other, the rotation is produced. But the effect is improved by placing another magnet 90° from the first, having its poles in the contrary direction, and being connected with it by a bar of soft iron placed beneath them. With this arrangement, the rotation will be from right to left, or from left to right, according to the position of the lamp.†

Upon the subject of thermo-electricity, we refer the reader to Becquerel's *Traite de l'Electricité et du Magnetisme*, (ii. 34.) He has shown that heat does not exalt the electricity of a charged Leyden phial, that when a single wire in communication with a delicate condenser is heated in one point, while the rest is kept cool, the hot portion becomes positively electric, while the next portion is negative, and that these two states make their way along the wire as the heat advances, and that thus a current of electricity is evolved. He has also investigated with much ingenuity the intensity of currents induced by difference of temperature in different metals soldered together, and though he has not been successful in discovering

* Annals of Philosophy (2d Series), vi. 179. † Ibid. p. 436.

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the law which this intensity follows, he has determined several points of considerable importance.

But heat not only alters the conducting power of bodies, and induces currents of electricity in conductors; there are some bodies, which, when heated, become charged with electricity, both positive and negative, and which when thus excited, retain their charge for a considerable time. The most remarkable of these bodies is the *tourmalin*.

The tourmalin.

The tourmalin is a hard crystallized mineral, which occurs in granite, and mica slate. It has been observed also imbedded in dolomite, at St Gothard. The primary form of the crystal is an obtuse rhomboid, but it occurs most commonly crystallized in three, six, or nine-sided prisms, terminated sometimes by three-sided, sometimes by six-sided pyramids, and sometimes by other terminations, which are minutely described by crystallographers.

This mineral appears to have been known to the ancients, and seems to be mentioned by Theophrastus, under the name of lyncurium, λυγκυριον.* According to Theophrastus, it has a fire colour, and is formed from the concreted urine of the lynx. The first modern writer who notices it is Lemery, who had obtained a specimen from Ceylon, which he exhibited to the members of the French Academy under the name of a magnet.† He notices, though rather inaccurately, the property which it has of attracting light substances to it. It was employed by jewellers for ornamental purposes, and they observed that when heated, it acquired the property of attracting light bodies, as ashes, to itself; on that account it was called by the Dutch *aschentrecher*, and by the Germans *schenzieher*, (*attractor of ashes*.) Æpinus having been informed of this property by Lehman, naturally referred it to electricity. He was fortunate enough to procure a tourmalin of considerable size, which

* Περι λιθων, c. 50. "The *lyncurium*, which is likewise used for engraving seals on, and is of very solid texture, like a stone. It has also an attractive power like that of amber, and is said to attract not only straws and small sticks, but even copper and iron if in small fragments. (λεπτα). Such is the statement of Diocles."

† Hist. de l'Academie Royal, 1717, p. 7.

enabled him to investigate the electrical phenomena exhibited by this curious mineral. The result of his observations was published in the *Memoirs of the Berlin Academy* for 1756.*

In the year 1766, Bergman published in the Memoirs of the Stockholm Academy, a memoir on the tourmalin, in which he describes its electrical characters with accuracy and in sufficient detail. Very little has been added to this important paper, which details all the electrical properties of the tourmalin with great accuracy.† Canton had already in 1759, mentioned the important fact, that if a tourmalin while possessed of polar electricity be broke in two at the neutral point, each fragment will possess two poles electric, like the whole crystal before it was broken.‡ Among the more modern investigators of the electric properties of the tourmalin, may be mentioned M. Becquerel§ and Professor Forbes‖ of Edinburgh.

Electricity evolved by the tourmalin.

When the tourmalin is heated, one of the ends of the crystal becomes positive and the other negative, and when the stone begins to cool, both extremities change the state of their electricity. Bergman showed that the electrical state of the tourmalin when it is heated may vary in the five following ways :—

	One of the poles.	The other pole.
1	+	—
2	+	+
3	—	—
4	+	0
5	—	+

The poles assume the same state (whether positive or negative), if one of them be in the act of cooling while the other is heating.

One pole becomes positive while the other continues neutral

* Collection Academique, xiii. p. 247.

† This paper appeared also in the *Philosophical Transactions* for 1766, p. 236. An interesting historical account of the tourmalin will be found in *Kong. Vetenskaps Acad. Handlinger* for 1766, p. 89.

‡ See Priestley's History of Electricity, p. 323.

§ Ann. de Chim. et de Phys. xxxvi. 1.

‖ Transactions of the Royal Society of Edinburgh.

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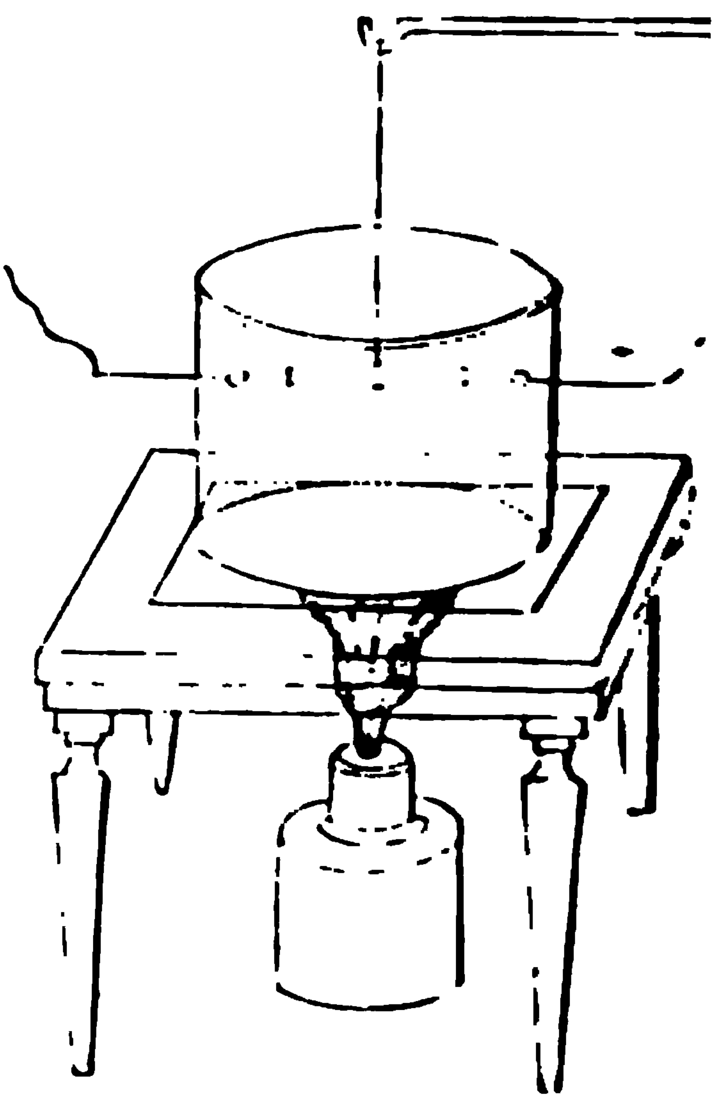
One pole becomes plus and the other minus, when the wh[ole] crystal is exposed at once to an equal heating or cooling pr[o]cess.

Hauy first observed that those crystals of tourmalin on[ly] become electric by heat, whose terminations deviate from t[he] law of symmetry.*

To observe all the electrical phenomena of the tourmalin which have just been stated, we place the tourmalin in a slip of paper suspended horizontally within a glass cylinder by a single thread of raw silk; this cylinder reposes on a metal plate which is heated by means of a spirit lamp below it. In proportion as the inside of the cylinder becomes heated, the tourmalin becomes electric, in consequence of the elevation of its temperature. We



have only to present to it a body weakly charged with ele[c]tricity to observe the attractions and repulsions. A thermo[-] meter placed within the cylinder indicates the temperatur[e]. The following are the results obtained by M. Becquerel wit[h] a brown tourmalin, slightly transparent, about 1·2 inch long, and 0·12 inch in diameter.†

At 86°, the electric polarity began to be sensible on the ap[-] proach of a body feebly electrified. It continued to the tem[-] perature of 300°, and even beyond it, provided that th[e] temperature did not cease to increase; for as soon as it be[-] came stationary, the polarity vanished. It appeared aga[in]

* Traite de Mineralogie, iii. 44.

† Traité de l'Electricité, ii. p. [illegible]

but of the opposite kind when the temperature began to sink. The charge is very quick, yet the intensity of each pole is not as the rapidity of heating or cooling. Chap. XI.

Having thus seen what passes when every part of the tourmalin is equally heated or cooled at the same time, let us see what happens when one of the extremities receives more heat than the other. Enclose each end of the tourmalin in a small glass tube, filling the end as exactly as possible, then tie the tourmalin by the middle to a glass tube by means of a platinum wire. If we now heat one of the ends—the one, for example, which becomes positive while heating—this end will acquire heat from the tube in which it is enclosed, will assume the same temperature with it, and will then begin to fall. As long as the other end does not acquire heat, it will have only one pole, namely, the heated end, which will be positive while heating, and negative while cooling, while the other end remains neutral. This curious phenomenon constitutes a kind of paradox in electricity. In all other cases the two electricities are always evolved together; but here one appears without any trace of the other. It would seem from this, that when we heat the two ends of a tourmalin unequally, each extremity assumes a state of electricity independent of the other: for if the other extremity begins to acquire or to lose heat, it assumes a positive or negative state, but remains neutral as long as the temperature continues unchanged. The end not heated may be kept cool by putting it into a small tube filled with ice.

Tourmalins, even from the same locality, vary so much in their electric intensity, that it is necessary to try each before we can be sure of its activity. The black opaque tourmalins, usually called *schorls*, are seldom electric;* but I have found brown, green, and red transparent crystals possessed of electric properties. Two modes are followed to render them electric, namely, *slow* and *rapid* heating and cooling. The first is exemplified in Becquerel's experiment above described. The second mode consists in transporting the cold tourmalin into

* Becquerel assures us, that certain black and opaque tourmalins have decided electric properties.

 a hot place, and observing what happens while it is heating. It is then taken and allowed to cool by putting it into a cold place. Tourmalins of great energy become electric by both modes, those of little energy only by the second method; while there are some that cannot be rendered electric by either the one or the other.

M. Becquerel tried a tourmalin, very slightly translucent on the edges, 4·15 inches in length, and 0·79 inch in diameter, but could not succeed in obtaining electric excitement by any method which he could devise. He found that a tourmalin which could not be made electric by heat, being broken in two, each half of it could be readily excited in the usual manner. It would appear, from his experiments, that a certain length of crystal is incompatible with electric excitement, and that short tourmalins (provided they be sufficiently translucent) have more intensity than long ones.

When the tourmalin is once excited by heat, it retains its electricity for a long time, if care be taken to place it upon non-conductors. Æpinus has found it electric after an interval of six hours.*

Thus the tourmalin possesses the characters of a charged Leyden phial, one of its poles being positive, the other negative. Æpinus showed that if these poles be coated with tinfoil, they may be discharged precisely in the same way as a Leyden phial. The great difference between the charged tourmalin and the Leyden phial, is the method of charging. In the tourmalin, in order to produce a charge, we have nothing more to do than to apply equable heat. Can any explanation of this curious mode of charging be offered?

Explanation of these phenomena.

If we melt sulphur in an iron cup, and allow it to cool after insulating the cup, we shall find that the sulphur is charged with negative electricity, and the iron with positive. Something similar to this must take place in the tourmalin. It must be composed of a number of elements similar, as far as their electrical properties are concerned, to the sulphur and iron:

* These facts, as stated by Æpinus, if accurate, seem inconsistent with the statement of Canton and Becquerel, that the electricity is only developed while the stone is changing its temperature.

and it can be shown that if we suppose the tourmalin so constituted, (since the stone is a non-conductor,) heat would produce the very electrical phenomena which distinguish and have given celebrity to this mineral.

Suppose we have a number of glass plates coated at the centre on each side with a piece of tinfoil, while the rest of the disc is covered with varnish to prevent the bad effects of moisture upon the glass. Let these plates be placed parallel to each other upon a non-conducting body about an inch asunder, and let the contiguous plates communicate with each other by means of a slip of tinfoil pasted on. From the last glass plate a chain passes communicating with the ground, while the first plate communicates with the prime conductor of an electrical machine. By means of this machine let the plates be charged with electricity. Let the apparatus be now insulated and examined. It will be found similar to the tourmalin. The end next the electrical machine will be charged with positive electricity, the end farthest off will be charged with negative electricity, while the central portion will be neutral. Indeed, neither the positive nor negative electricity extends sensibly beyond the 4th plate from either extremity. The 24 middle plates are sensibly neutral. If this apparatus, while thus charged, were broken in two, we should find that both the halves possessed exactly the characters of the whole pile. The two extremities of each half would be charged with positive and negative electricity, while the central portion would be neutral. Such a pile, therefore, represents precisely the state of the excited tourmalin; and therefore serves to throw light upon the nature of that stone.

The tourmalin is not the only mineral which acquires electricity by heat. There are several others, some of the most remarkable of which it will be proper to enumerate:—

Boracite similar to the tourmalin in its electric properties.

There is a hill of sulphate of lime, called Kalkberg, situated near Lunebourg, in the dutchy of Brunswick, in which small cubic crystals are found. These cubes are white, have a specific gravity of 2·566, and are composed of two atoms of boracic acid combined with one atom of magnesia. They are distinguished among mineralogists by the name of *boracite*. If we examine the cubic crystals of boracite, we shall find that

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only four of the solid angles are complete, constituting alternate angles, placed at the extremity of two opposite diagonals at the upper and lower surface of the cube. The other four solid angles are replaced by small equilateral triangles. When the boracite is heated, all the perfect solid angles become charged with *negative* electricity, while all the angles replaced by equilateral triangles become charged with *positive* electricity. So that the boracite has eight poles; four positive, and four negative. These are obviously the extremities of four diagonals connecting the solid angles with each other. One extremity of each of these diagonals is charged with positive, and the other extremity with negative electricity. In general the electricity of boracite is not so strong as that of the tourmalin. This curious law of the excitability of the boracite and of its eight poles, was discovered by Hauy, in 1791.*

Other electric minerals.

Axinite,† *mesotype*,‡ and the *silicate of zinc*,‡ are also minerals, which become electric when heated, and which, like the tourmalin, exhibit two opposite poles, the one positive, the other negative. It is not every crystal of axinite and mesotype which possesses this property; but those only which are destitute of symmetry: that is to say, one of whose extremities is shaped differently from the other. No doubt this remark applies also to the silicate of zinc; though the crystals of that mineral being usually acicular it is not so easy to determine, by observation, the degree of symmetry which they may possess.

The *topaz*,§ *prehnite*,‡ and the titaniferous mineral called *sphene*, are also capable of being excited by heat, and have two opposite poles like those already mentioned.

Sir David Brewster has added to this catalogue of bodies becoming electric by heat, the following substances:—‖

Scolezite	Diopside
Mezolite	Red and blue fluor spar
Calcareous spar	Diamond
Yellow beryl	Orpiment
Sulphate of barytes	Analcime
Sulphate of strontian	Amethyst
Carbonate of lead	Quartz of Dauphiné

* Hauy's Mineralogie, ii. 60, 2d Edition. † Brard. ‡ Hauy. § C[illegible]

‖ Ann. de Chim. et de Phys. xxviii. 162.

Idocrase	Sulphate of iron
Mellite	Sulphate of magnesia
Native sulphur	Prussiate of potash
Garnet	Sugar
Dichroite	Acetate of lead
Rochelle salt	Carbonate of potash
Tartaric acid	Citric acid
Oxalate of ammonia	Chlorate of potash
Soda-sulphate of magnesia	Chlorate of mercury
Sulphate of ammonia	

But M. Becquerel has thrown some doubts about the accuracy of the method employed by Sir David Brewster, to detect the existence of pyro-electricity in these bodies.* The probability is, that those crystals only become electric by heat which derogate from the law of symmetry.

CHAPTER XII.

OF ELECTRICITY BY PRESSURE.

It is natural to expect that the dilatation and compression of the particles constituting the surfaces of bodies will have an effect upon the development of their electricity. Æpinus was the first who ascertained the truth of this presumption by actual experiment. He pressed two plates of glass together, taking care to avoid friction, and found, when they were separated, that one of them was positive and the other negative. Hauy observed that Iceland spar and several other crystallized minerals, became electric by simple pressure between the fingers. He found the same property in several other minerals, though not so much marked as in Iceland spar. The topaz, euclase, arragonite, fluor spar, carbonate of lead, and rock crystal, give signs of electricity when pressed.† M. Libes fixed an insulating handle to a metal disc, and pressed it (holding it by the

* Traité de l'Electricité, ii. 69.

† Hauy; Mineralogie, i. 188, 2d edition.

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handle) against a piece of gummed taffeta. The taffeta acquired positive electricity, while the metallic disc acquired negative electricity.* The effect increases with the pressure; but it ceases altogether as soon as the taffeta loses its glutinosity, which renders it easily compressible.

This subject has lately engaged the attention of M. Becquerel, who has made a vast number of experiments on different bodies, and has drawn this general conclusion from them all:

When two elastic bodies are pressed they become differently excited.

—" When two substances, of what nature soever, provided one of them be elastic, are insulated, and pressed against each other, one of them becomes charged with positive, and the other with negative electricity. But when the compression is removed, these two different states cannot be observed, unless one of the bodies, at least, be a bad conductor of electricity."

The mode chosen by Becquerel to obtain these results, was to form the substances to be tried, into small discs, about one-tenth of an inch thick, to fix each to a varnished glass rod by way of handle, to take one of these handles in each hand, and squeeze the two discs together. After separating them, each disc is to be presented to a delicate electrometer. A single pressure is sufficient to repel the small disc of Coulomb's electrometer; but by repeating the contacts any electrometer whatever may be charged. Suppose, for example, two insulated dics, the one of *cork*, the other of *caoutchouc*. After the pressure, the cork has become positive, and the caoutchouc negative. When cork is pressed against *the skin of an orange*, it becomes positive, and the orange skin negative. When orange skin is pressed against caoutchouc it becomes positive, while the caoutchouc becomes negative.

Iceland spar, sulphate of lime, fluor spar, sulphate of barytes, when pressed against cork, become positively electrified, while the cork becomes negatively. But cyanite and retinasphaltum, when pressed against cork, acquire negative electricity, the cork becoming positive. In like manner, pitcoal, amber, copper, zinc, silver, &c., when pressed against an insulated disc of cork, become negative.

* Ann. de Chim. et de Phys. xxii. 5.

When one only of the discs is insulated while the other communicates with the ground, the insulated body will have acquired, after pressure, the electricity belonging to it; but the non-insulated body will exhibit no signs of electricity whatever. Thus, an insulated disc of cork, pressed against Iceland spar, selenite, fluor spar, &c., acquires negative electricity; but when pressed against copper, zinc, and other similar substances, it acquires positive electricity. Even fruits, the orange for example, when gently pressed against an insulated disc of cork, communicate an excess of positive electricity. In proportion as the fruit dries, the electric properties of the cork diminish. When ripeness has given the orange all the elasticity of which it is susceptible, and before the surface becomes moist from decomposition, then the electricity from pressure is the greatest possible.

But they must be insulated.

Insulated cork pressed against any part of the animal body, provided it be not moist, receives an excess of negative electricity. The hair and down of animals produce nearly as much electricity as Iceland crystal would do, but of the contrary kind.

The imperfect liquids, when compressible, give analogous results. Cork pressed lightly against inspissated oil of turpentine, acquires negative electricity.

It is not necessary that the substances pressed against each other be of different natures; when two discs composed of the same materials, as skin, amadou, &c., are pressed against each other, the one acquires negative and the other positive electricity.

Electricity thus evolved continues a long time.

When electricity is thus evolved in bodies by pressure, it is preserved for a considerable time, provided the bodies be non-conductors. Hauy found that Iceland spar still gave signs of electricity after an interval of eleven days. Sulphate of barytes of Royat, unless it be well insulated, parts with the electricity evolved in it almost immediately. But an insulated crystal of it exhibited signs of electricity after an interval of half an hour. The time during which the electrical properties are preserved, is probably inversely as the conducting power of the electrified body. This preservation of electricity continues even though

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the body be surrounded by moist air, or even though it be in contact with moist substances. It would appear from this, that bodies rendered electric by pressure, are in the same circumstances with a charged Leyden phial. The internal surface of the body is probably charged with the opposite electricity of the external surface; and the action of these opposite electricities on each other, prevents that on the external surface from being dissipated.

Electricity appears only when the bodies are separated.

As long as the two bodies pressed against each other are in contact, neither of them gives any symptoms of electricity. The better conductors they are, the more rapidly they must be separated from each other, after pressure, in order that each may retain the peculiar kind of electricity which has been accumulated in it by the pressure. When a disc of cork is pressed against an orange, if we separate the two substances rapidly, the cork will be found to possess a considerable excess of positive electricity. This excess diminishes the more slowly the two substances are separated from each other, and when we separate them very slowly, all electrical symptoms disappear.

It would appear from all this, that when two surfaces are pressed against each other, one of them always acquires an excess of positive electricity, while the other acquires an excess of negative electricity. These two opposite electricities just neutralize each other. Now, it is a curious and inexplicable circumstance, (unless we consider electricity as a property) that as long as the pressure is kept up, even though the two bodies pressed against each other be good conductors, the two electricities though attracted towards each other, never actually combine. But whenever the pressure is withdrawn, unless the bodies be instantly separated from each other, the two electricities combine and neutralize each other completely, unless in the case when at least one of the pressed bodies is a non-conductor.

Heat plays a considerable part in these developments.

There can be no doubt that heat plays a considerable part in the development of electricity by pressure. It has been long known, that the higher the temperature of any body is raised, the greater tendency has it to acquire negative elec-

tricity when rubbed against any other body. In the same manner, when we continue to elevate the temperature of Iceland spar, we at last bring it into such a state, that it acquires negative electricity when pressed against a disc of cork. If we take a piece of well dried cork, and cut it in two, by means of a very sharp knife, and then press the two cut surfaces against each other, it frequently happens that however hard the pressure be, and however rapidly we separate the two surfaces, neither exhibits any signs of electricity after the separation. But if we slightly heat one of the pieces of cork, by holding it near the flame of a candle, and renew the pressure, we shall find each surface possessed of a different kind of electricity. Two pieces of Iceland spar, of the same temperature, do not become electric by pressure; but if one of them be made hotter than the other, the electricities are immediately evolved. It is probable from this, that when two discs of the same substance are pressed against each other, it is necessary that their temperatures should differ, otherwise no electricity will be evolved.

The intensity of the electricity proportional to the pressure.

It would appear from the experiments of M. Becquerel that the intensity of electricity, measured by Coulomb's balance, is proportional to the pressure; that is to say, that a double pressure will produce a double intensity, a triple pressure a triple intensity, and so on. The following tables exhibit the intensities of electricity evolved by the pressure of various bodies against each other. The intensity of pressure was measured by the weight attached to one end of a balance forcing up the other end which produced the pressure:—*

Cork pressed against Iceland spar.

Pressures.	Intensity of electricity.
1	1·5
2	3·4
3	5·6
4	6

* A detailed account of the apparatus, illustrated by figures, may be seen in the Ann. de Chim. et de Phys. xxii. 21.

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Cork pressed against polished sulphate of barytes crystals.

Pressures.	Intensity of electricity.
1	1·05
2	2·1
3	3·1
4	4·2
6	6·3

Cork against polished rock crystal.

Pressure.	Intensity of electricity.
4	3·9

Cork against sulphate of lime.

Pressure.	Intensity of electricity.
4	1·9

These tables show also the difference in the absolute quantity of electricity evolved by the same extent of pressure applied to different bodies.*

CHAPTER XIII.

OF ELECTRICITY BY CONTACT.

This branch of electricity originated from a dispute between Galvani and Volta, about the year 1790. Galvani accidentally discovered, that when the hind legs of frogs, deprived of their skin, but having the lumbar nerve attached to them and laid bare, are placed upon pieces of glass, the lumbar nerve being previously enveloped in a piece of copper foil; if we take a piece of zinc, and placing one end of it on the muscles of the leg, we bring the other in contact with the copper foil, the muscles of the limb are thrown into violent convulsions. These convulsions are renewed whenever the metals are brought into contact for a certain time after death. The

Convulsions in the muscles of a frog.

* Ann. de Chim. et de Phys. xxii. 5.

'hen the irritability of the muscles is exhausted, and :es place the sooner, the oftener the convulsions are ·d by bringing the two metals in contact. Galvani l these convulsions to an *animal electricity*, which, in his , exists in all animals, and is the cause of muscular

Ch. XIII.

Ascribed to animal electricity.

When Volta repeated the experiment, he was struck e necessity of two metals for its success. He was , in consequence of this, to ascribe the phenomenon non electricity, and to affirm that when two metals are .n contact with each other, one acquires vitreous, and ɜr resinous electricity. To verify this opinion, he pre- wo circular polished plates, the one of copper and the f zinc. To the centre of each of these, was cemented ;hed glass handle. The two plates were brought into by means of these handles, and separated so as to pre- parallel position with respect to each other. These .ade to touch the plates of a condenser, the contact ɜnewed several times. After touching each plate to re- ; to a neutral state, it was found that the zinc pos- an excess of positive electricity, while the copper ɜd an excess of negative. Galvani did not accede to inion of Volta. He repeated his experiments, and .ed in producing the convulsions by means of a single but only when the energy of the limb was very great. ı this last case, he succeeded in producing the convul- imply by bringing other animal matters in contact with bar nerve, and the crural muscles of the frog. But .nswered these objections, by showing that the smallest y of any foreign metal sufficed to enable a single : plate to produce the convulsions.

To common electricity.

n zinc is brought into contact with copper or silver, and ɜparated by means of an insulating handle, the zinc is 'ositive and the copper or silver negative. It is with ıetals that the different states of electricity after con- .y be most easily perceived. If *iron*, or *manganese*, or *umbago*, be substituted for the zinc plate, the result is ıe. I presume that if *cadmium* were substituted for would also be found positive after the separation of the

Two insulated metals brought into contact and separated are in different electric states.

Part II. metallic plates. Gold, platinum, palladium, or even mercury, may be substituted for the copper or silver. When tin or lead is substituted for copper, the different states of the metals are not easily recognised, but in this case also the zinc is positive, and the tin or the lead negative.

But when we take metals not liable to oxydation, as gold and platinum, no disengagement of electricity is ever observed in consequênce of their mutual contact, how sensible soever the electrometer be which we employ. This leads to the suspicion that the electricity observed by Volta, when he made use of copper, may have been owing to the chemical action occasioned by the contact of his wet finger on that very oxydizable metal. It is also well known that when a plate of gold, and another of platinum, both in contact with a most delicate electroscope, or plunged into a liquid which has no action on them, no signs of electricity are ever exhibited; but if the liquid be a solvent (as aqua regia), a very sensible electric action ensues.

Platinum and gold are positive with respect to binoxide of manganese and plumbago, and produce no electrical effect when placed in contact with red oxide of copper, persulphuret of iron, finery cinder, oligiste iron ore, &c. Binoxide of manganese and plumbago are negative with respect to these last substances. In general binoxide of manganese is negative with respect to all other bodies. Arsenical cobalt and gold produce also marked effects: the former is negative, the latter positive. Whether these evolutions of electricity be owing simply to contact, or whether any chemical action takes place when the electricity is evolved, has not yet been determined. We refer the reader to the ingenious experiments of M. Delarive, who has endeavoured to prove that chemical action takes place in all these cases of apparent contact.*

A considerable number of experiments on the electricity by contact was made by Sir H. Davy. When oxalic, succinic, benzoic, or boracic acid, perfectly dry, either in powder or crystals, are touched upon an extended surface with a plate

* Ann. de Chim. et de Phys. xlvi. 286.

copper insulated by a glass handle, the *copper* becomes *positive*, and the *acids negative*. When zinc or tin is substituted for copper the effect is the same. Phosphoric acid perfectly dry, when applied to copper becomes negative, and the copper positive. When metallic plates are made to touch dry lime, strontian, barytes, or magnesia, these alkaline bodies become *positive*, the metal negative. With soda the effect is the same. Potash attracts moisture so rapidly that the experiment cannot be tried with it. When sulphur is applied to polished lead, or to any other metal, it becomes positive.*

Elements of bodies decomposed by electricity either positive or negative.

It has been ascertained that electricity, when employed in the way to be described hereafter, and known by the name of the Voltaic battery, is capable of decomposing compound bodies, and the decomposition takes place according to a particular law. When two platinum wires attached to the two poles of the battery are plunged into a vessel of water, the water is reduced into its elements, and the *oxygen* is always extricated from the wire attached to the *positive* pole, while the hydrogen rises from the wire attached to the *negative* pole. When the wires are plunged into a strong solution of muriatic acid, the chlorine is accumulated round the positive wire, and the hydrogen round the negative. The law according to which *hydrobromic* and *hydriodic* acids are decomposed is the same: the bromine and iodine are attracted to the positive pole, while the hydrogen is attracted to the negative pole. When chloride of sodium or potassium is decomposed, the chlorine passes to the positive pole, while the sodium and potassium pass to the negative pole. Were a compound of sulphur with oxygen to be decomposed, the oxygen would attach itself to the positive pole, while the sulphur would attach itself to the negative pole. But with sulphuret of zinc or of iron the case would be different. The sulphur would collect round the positive pole, and the metal round the negative pole. When salts are decomposed, the acid is attracted to the positive pole, and the base to the negative.

Now, as bodies are attracted by those in a different state of

* Phil. Trans. 1807, p. 34.

Part II. excitement from themselves, it follows that oxygen, chlorine, bromine, and iodine, and acids, would not be attracted to the positive pole, unless they themselves were in a negative state; nor would hydrogen and bases be attracted to the negative pole unless they were in a positive state. From this it has been concluded that bodies which have an attraction for each other are in opposite states of electricity, and that it is to these opposite states that their attraction for each other, and their union with each other, is owing. The current of electricity destroys their union by bringing them into the same electrical state. In consequence of this view, which is at least exceedingly ingenious and plausible, bodies have been divided into two sets, those which are negative, and those which are positive. The following table exhibits a list of the negative bodies, beginning with those which possess the negative property in the highest degree, and terminating with those in which it is lowest:—

Table of negative bodies.

Oxygen	Molybdenum
Chlorine	Chromium
Bromine	Tungsten
Iodine	Boron
Sulphur	Carbon
Phosphorus	Antimony
Selenium	Tellurium
Arsenic	Columbium
Titanium	Silicon.

The following table exhibits a list of the positive bodies beginning with the one in which the property is weakest, and ending with the one in which it is strongest:—

Table of positive bodies.

Gold	Uranium?
Platinum	Iron
Palladium	Cadmium
Osmium?	Zinc
Iridium	Manganese
Rhodium	Aluminum
Mercury	Thorinum
Silver	Yttrium
Copper	Glucinum

Nickel	Magnesium
Cobalt	Calcium
Bismuth	Strontium
Tin	Barium
Zirconium	Lithium
Lead	Sodium
Cerium	Potassium.

It is not easy to decide where hydrogen should be placed. Compared with oxygen it is strongly positive; but it combines with the potassium, and must with respect to it be negative. The bodies nearest the head of the first list being most powerfully negative, and those nearest the bottom of the second list being most powerfully positive, have the greatest chemical affinity for each other. Bodies in the same list have but little affinity for each other, those towards the bottom of the first list have but little affinity with those towards the top of the second list; however, the bodies in the same list are not destitute of affinity for each. Thus, sulphur combines readily with arsenic, because these two bodies assume different states with respect to each other. When we decompose sulphuret of arsenic, the sulphur is attracted to the positive pole, and the arsenic to the negative; showing that the former is in a negative state, and the latter in a positive. It is for this reason that almost all the substances constituting the first list, are capable of uniting with each other as well as with those of the second. Now, it deserves attention, that when the bodies constituting the first list unite with each other, they constitute *acids* or substances which act the part of acids; when they combine with the substances constituting the second list, they constitute *bases* or substances which act the part of bases. All the acids are combinations of the negative bodies with each other, all the bases are compounds of the negative bodies with the positive. I have left out azote because it is not easy to say where it ought to stand; but it belongs undoubtedly to the class of negative bodies, and should stand probably before *sulphur*.

Hydrogen I have purposely omitted. It constitutes *acids* by combining with the greater number of the negative bodies;

Part II. but we are ignorant at present of any compound which it forms with any of the positive bodies, excepting with potassium, which, according to the experiments of Gay-Lussac and Thenard, absorbs hydrogen gas, and forms a compound. But this combination has not succeeded in the hands of other experimenters.

CHAPTER XIV.

OF ELECTRICITY BY CLEAVAGE.

For the few facts hitherto ascertained respecting the evolution of electricity by the cleavage of crystallized minerals, we are indebted to M. Becquerel.*

When we cleave a plate of mica as rapidly as possible in the dark, we generally perceive a feeble phosphorescent light. If we fix with mastic an insulating handle upon each of the faces of this plate, we may ascertain that each of the slices separated is in a contrary state of electricity, the intensity of which increases with the rapidity of the separation. These results are always obtained, however thin the plate of mica may be: hence, probably, they would take place how small soever the particles of mica were that we separated from each other.

Foliated talc of St Gothard, transparent sulphate of lime, topaz, sulphate of barytes, felspar of St Gothard, and all crystallized minerals which are bad conductors of electricity, give the same result.

Before making these experiments, we must free the mineral from all water mechanically lodged in it, and we must be sure that the plate of mica, sulphate of lime, &c., are not already partially split, which would of course prevent the action.

Two plates of mica already separated, if they be pressed

* Traité de l'Electricité, ii. 111.

together anew and separated, are each charged with the opposite electricity; but this state lasts but a very short time. If we wish to restore it, we must raise a little the temperature of that plate which has assumed negative electricity.

A card paper doubled down presents a similar effect when the two fragments are separated. Here the filaments composing the card are united to each other by a certain force which is destroyed. This has some resemblance to the cleaving of foliated minerals. When crystallized minerals are triturated in a mortar, electricity cannot be evolved, because the two electricities immediately combine again in consequence of the contact of portions of the mineral in different electrical states.

Pressure it is obvious causes a mechanical approach of the particles to each other, in some cases causing the two bodies to adhere together. In this last case the separation ought to produce the same electrical effects as when a foliated mineral is split.

CHAPTER XV.

OF ELECTRICITY BY CHEMICAL COMBINATIONS AND DECOMPOSITIONS.

Ever since the discovery of the identity of lightning and electricity, the attention of electricians has been turned to the accumulation of electricity in the atmosphere; and various causes for such an accumulation have been assigned. The opinion of Volta has been most commonly adopted. According to him, whenever a body changes its state, it becomes electric. Now, water is continually ascending into the atmosphere in the state of vapour, or falling from it in the state of rain. By these continual changes of state, which this fluid undergoes, Volta supposed that the accumulation of electricity in the atmosphere was chiefly produced. This opinion was

Evaporation.

Part II.

Does not evolve electricity.

Electricity evolved by combustion of charcoal.

verified by Lavoisier and Laplace; but when Saussure peated the experiments, he was unable to obtain any satisf tory results. M. Pouillet has recently examined the subj with much care, and has found that no sensible quantity electricity is evolved when water changes its state, unless at same time some chemical action more or less vigorous acc panies the change. But whenever two gaseous bodies ur with each other, or a gaseous body with a solid body, one the uniting bodies always gives out positive electricity, a the other negative electricity.* These experiments being great importance, both for understanding the sources of atn spherical electricity, and for determining the kind of elect city possessed by those bodies which have a chemical affin for each other, it will be worth while to state them somewl particularly. When charcoal is burnt it sometimes gives positive, and sometimes negative electricity, and sometin no electricity at all. This depends upon the way in whi the combustion is conducted. To obtain constant results, Pouillet proceeded in the following manner:—He took a pi of charcoal of such a diameter that it could receive the fo of a cylinder whose bases were nearly plain. This piece charcoal was placed vertically, two inches and a half, or thr inches below a plate of brass which rests upon one of t discs of the condenser. The charcoal communicated wi the ground, and was lighted at its superior base, taking ca that the fire did not reach the lateral surface. A current carbonic acid rises and strikes against the plate, and in a fe minutes the condenser is charged. The electricity which receives from the carbonic acid gas is always *positive*. If th plane be allowed to communicate to the sides of the charco or if it be inclined so that the carbonic acid formed ma slide up along the base of the charcoal, no sensible effect obtained.

To obtain the electricity which the charcoal itself takes combustion, M. Pouillet placed its inferior end directly up the disc of the condenser, and then lighted its superior bas

* Ann. de Chim. et de Phys. xxxv. 401.

In a few minutes the condenser was charged with negative electricity. From these experiments, we learn that when charcoal is burnt, it becomes charged with negative electricity, while the carbonic acid evolved is charged with positive electricity. Now, the combustion of charcoal is the combination of it with oxygen, so as to constitute carbonic acid. According to Pouillet, during this combination, the oxygen gives out positive electricity, which is found in the carbonic acid gas, while the charcoal gives out negative electricity, which is found in the portion of the charcoal not yet burned. Now, when the carbonic acid gas is again decomposed into its elements, the oxygen takes back positive electricity, and the carbon negative electricity.

And hydrogen.

The flame of hydrogen gave contradictory results with respect to its electricity, as had been the case also at first with charcoal. In the course of a few minutes it gave indications both of positive and negative electricity, very intense and very weak indications, and often it was impossible to obtain any indication at all. But these difficulties were at length overcome by M. Pouillet in the following manner:—

The hydrogen gas was made to flow out of a glass tube. The flame was vertical, having a breadth of 4 or 5 lines, and a height of about three inches. A coil of platinum wire was employed to conduct the electricity from the flame to the condenser. When this coil was so much larger than the flame as to enclose it, and to be distant from its external surface about 4 inches, signs of positive electricity make their appearance. These signs become more and more intense as the distance diminishes; but when the coil becomes so small as to touch the flame, the electrical signs become weak and uncertain. Thus it appears that round the flame of hydrogen, there is a sort of atmosphere at least 4 inches in thickness, which is always charged with positive electricity.

If a very small coil of platinum wire be placed in the centre of the flame, in such a manner, that it is enveloped on all sides; and made to communicate with the condenser, that instrument becomes immediately charged with negative electricity. Thus it appears that the outside of the flame of hydro-

Part II. metallic plates. Gold, platinum, palladium, or even mercury, may be substituted for the copper or silver. When tin or lead is substituted for copper, the different states of the metals are not easily recognised, but in this case also the zinc is positive, and the tin or the lead negative.

But when we take metals not liable to oxydation, as gold and platinum, no disengagement of electricity is ever observed in consequênce of their mutual contact, how sensible soever the electrometer be which we employ. This leads to the suspicion that the electricity observed by Volta, when he made use of copper, may have been owing to the chemical action occasioned by the contact of his wet finger on that very oxydizable metal. It is also well known that when a plate of gold, and another of platinum, both in contact with a most delicate electroscope, or plunged into a liquid which has no action on them, no signs of electricity are ever exhibited; but if the liquid be a solvent (as aqua regia), a very sensible electric action ensues.

Platinum and gold are positive with respect to binoxide of manganese and plumbago, and produce no electrical effect when placed in contact with red oxide of copper, persulphuret of iron, finery cinder, oligiste iron ore, &c. Binoxide of manganese and plumbago are negative with respect to these last substances. In general binoxide of manganese is negative with respect to all other bodies. Arsenical cobalt and gold produce also marked effects: the former is negative, the latter positive. Whether these evolutions of electricity be owing simply to contact, or whether any chemical action takes place when the electricity is evolved, has not yet been determined. We refer the reader to the ingenious experiments of M. Delarive, who has endeavoured to prove that chemical action takes place in all these cases of apparent contact.*

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* Ann. de Chim. et de Phys. xlvi. 286.

copper insulated by a glass handle, the *copper* becomes *positive*, and the *acids negative*. When zinc or tin is substituted for copper the effect is the same. Phosphoric acid perfectly dry, when applied to copper becomes negative, and the copper positive. When metallic plates are made to touch dry lime, strontian, barytes, or magnesia, these alkaline bodies become *positive*, the metal negative. With soda the effect is the same. Potash attracts moisture so rapidly that the experiment cannot be tried with it. When sulphur is applied to polished lead, or to any other metal, it becomes positive.*

Elements of bodies decomposed by electricity either positive or negative.

It has been ascertained that electricity, when employed in the way to be described hereafter, and known by the name of the Voltaic battery, is capable of decomposing compound bodies, and the decomposition takes place according to a particular law. When two platinum wires attached to the two poles of the battery are plunged into a vessel of water, the water is reduced into its elements, and the *oxygen* is always extricated from the wire attached to the *positive* pole, while the hydrogen rises from the wire attached to the *negative* pole. When the wires are plunged into a strong solution of muriatic acid, the chlorine is accumulated round the positive wire, and the hydrogen round the negative. The law according to which *hydrobromic* and *hydriodic* acids are decomposed is the same; the bromine and iodine are attracted to the positive pole, while the hydrogen is attracted to the negative pole. When chloride of sodium or potassium is decomposed, the chlorine passes to the positive pole, while the sodium and potassium pass to the negative pole. Were a compound of sulphur with oxygen to be decomposed, the oxygen would attach itself to the positive pole, while the sulphur would attach itself to the negative pole. But with sulphuret of zinc or of iron the case would be different. The sulphur would collect round the positive pole, and the metal round the negative pole. When salts are decomposed, the acid is attracted to the positive pole, and the base to the negative.

Now, as bodies are attracted by those in a different state of

* Phil. Trans. 1807, p. 34.

Part II.

Consequently the carbonic acid gas, which disengages its during the germination of seeds, is charged with posit electricity, and is therefore precisely in the same state as carbonic acid gas formed by combustion. This experim was several times repeated with success. But the electric cannot be recognised unless the weather be exceedingly d or unless we dry the apartment artificially by introducing s stances which have the property of absorbing moisture.

These capsules being insulated, and the air being very d and the soil so dry that it is an imperfect conductor, it evident that the electricity would be retained. Accordin when the condenser was brought into a natural state after o observation, and if it was then replaced for experiment o during one second, it was found to be charged with elect city.

It is obvious enough that the gaseous fluids given out plants during the processes of vegetation, being charged wi electricity, must contribute to furnish no inconsiderable p tion of the electricity with which the atmosphere becom loaded. No doubt the carbonic acid gas evolved from anim by respiration is also charged with positive electricity: thou it would be somewhat difficult to determine the point actually charging a condenser in consequence of the moistu with which the expired air is always loaded.*

All chemical combinations and decompositions evolve electricity.

The electricity evolved by chemical combinations and d compositions was long a subject of dispute, but since the di covery of the connexion between electricity and magnetism, a the employment of magnetic needles to indicate the existen of electric currents, the evolution of electricity in such cas has become manifest and indisputable. When we plunge o after the other, the two ends of the copper wire of a multipli into nitric acid, a current of electricity is immediately evolve Gold or platinum wire connected with a multiplier whe plunged into nitric acid, gives no indication of the evolution

* The reader may consult with advantage an elaborate and most inge ous set of experiments by Sir H. Davy, to establish his own views, wh are rather inconsistent with those of Pouillet, in Phil. Trans. 1826, 398.

electricity. But if we add a few drops of muriatic acid to that portion of the nitric acid, which surrounds one of the ends of the wire, a current is induced, and the wire which is attacked by the aqua regia becomes negative while the other extremity is positive.

When an acid combines with an alkali, the former becomes positive, and the latter negative. When various substances are placed so as to come gradually in contact in tubes connected with a delicate multiplier, one always becomes positive and the other negative.

Nitric acid is *positive* with muriatic acid, acetic acid, nitrous acid, alkaline solutions, and solutions of sulphates, nitrates, chlorides, &c.

Nitric acid is *negative* with sulphuric acid, phosphoric acid, &c.

Phosphoric acid is *positive* with muriatic acid, sulphuric acid, nitric acid, and with alkaline and saline solutions.*

Phosphoric acid is the most electro-positive of all liquids.

A great deal of light has been thrown upon the electricity evolved during chemical decompositions and combinations, by the experiments of Mr Faraday.

He showed that when electricity from a common electrical machine is made to pass through solutions of salts, (*sulphate of soda* was the one used,) they undergo decomposition, the acid being evolved at the positive, and the alkali at the negative wire in contact with the solution.†

An opinion has been adopted by some, that in the construction of the Voltaic arrangement for the evolution of electricity, the presence of water in the imperfect conductor between the metallic plates is essential; but Faraday has shown that this is a mistake, and that a vast number of bodies rendered liquid by heat may be substituted for water, which indeed is one of the bodies worst adapted for the purpose, though from other circumstances the most convenient.

Chemical decomposition.

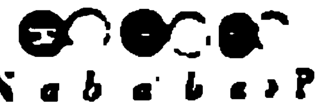
It has been supposed also pretty generally, that chemical decomposition is produced by electricity, by the attraction of

* Becquerel, Traité de l'Electricité, ii. 78.

† Phil. Trans. 1833, p. 676.

each pole for one of the constituents of the body decomposed. Thus water is decomposed, because the positive pole attracts the oxygen, while the negative pole attracts the hydrogen, and these two attractions increasing with the intensity, or at least with the quantity of electricity evolved, become at last greater than the chemical affinity which unites the atoms constituting water to each other, and when that is the case, decomposition must take place. If I understand Mr Faraday's ideas respecting chemical decompositions by electricity correctly, he conceives the decompositions to be produced by the chemical affinity between the particles being altered and diminished by the action of the electricity. Let N and P represent the negative and positive poles of a Voltaic battery, and let a b, a' b', and a'' b'', &c, be particles of water interposed between them, composed each of an atom of hydrogen, (a), and an atom of oxygen, (b). Mr Faraday supposes, that in consequence of the action of the electric current, all the particles of water in the way of the current are so arranged, that the hydrogen faces the negative and the oxygen the positive pole. He supposes farther, that in consequence of the electricity, the affinity of a for b is weakened, while the affinity of b for a' is increased. In consequence of this, a leaves b, and makes its appearance at N under the form of hydrogen gas. b unites to a', which in its turn unites to b', which combines with a'', while b'', in consequence of the weakened affinity of a'' for b'', separates at P, and makes its appearance under the form of oxygen gas.

Ascribed to electricity.



This progressive process of decomposition and combination has been long pretty generally admitted. But I do not see how the particle a could separate from b in consequence of the greater attraction of a for b, unless a at the same time were attracted by N. If we suppose that the attraction of N for a and of a for b, is greater than the attraction of a for b, a would undoubtedly separate from b; and if these mutual decompositions and combinations proceed equally from both poles, it is easy to see how the chemical decompositions take place, but not otherwise. Mr Faraday made a very ingenious ex-

periment. He decomposed sulphate of magnesia, and found that magnesia was deposited upon the plane which constituted the boundary line between a layer of pure water and a layer of solution of sulphate of magnesia. In this experiment the water was connected with the negative end of a Voltaic battery, and the sulphate of magnesia with the positive end: hence the water became the positive pole; and of course every property belonging to the positive pole must in this experiment have belonged to the water.

The probability seems to be, that throughout the whole extent of the Voltaic circle, all the particles in the way of the current (supposing them compounds), arrange themselves as we have supposed the particles of water to do; that similar decompositions and new combinations take place in every cell, and that the electricity evolved, is the consequence of these combinations and decompositions, and of nothing else, as was long ago argued by M. Fabroni and Dr Wollaston, and of late by Mr Faraday himself. He has rendered it extremely probable, that the same quantity of electricity always decomposes the same quantity of decomposable matter; so that the quantity of hydrogen evolved in a given time by the decomposition of water is an accurate measure of the quantity of electricity passing during that time through an active Voltaic battery.*

Mr Faraday, during the course of his electrical experiments, discovered that when a clean platinum plate was put into a mixture of oxygen and hydrogen gas, the gases gradually united forming water and consequently disappearing. When the platinum plate was made very clean by dipping it into sulphuric acid, it acted with so much energy upon the gases, that it gradually became red hot and caused the residue of the gases to explode.†

It had been previously shown by Dobereiner, that platinum black, or platinum in the state of a fine powder, acted in the same way. The experiments of Faraday throw some light on this curious property. He found that the plate was very apt to lose its power, but that when steeped in sulphuric acid of

* Phil. Trans. 1833, p. 692.

† Ibid. 1834, p. 58.

Part II.

specific gravity 1·336, or indeed when cleaned by various other methods, the power was recovered. Dalton has shown that the particles of hydrogen gas repel each other; that the case is the same with the particles of oxygen gas, but that the particles of hydrogen gas do not repel the particles of oxygen gas. Clean platinum does not repel, but rather attracts the particles of gases; and when the platinum is smooth and clean the two gases can approach very near its surface, if not come actually in contact with it. So situated, the particles of oxygen and hydrogen may come so near each other as to unite chemically, heat is evolved by the union, the rapidity of the process is increased till at last the platinum becomes red hot, and explosion is the consequence.

All compounds not decomposed.

Electric currents have not the property of decomposing all compounds as Davy supposed.

Boracic acid
Chlorides of sulphur, phosphorus, and carbon
Iodide of sulphur

are not decomposed in common circumstances.*

Faraday found also that

Chloride of antimony
Hydrocarbon
Acetic acid
Periodide of mercury
Ammonia and some other substances

are not decomposed by the Voltaic battery.

But with the exception of periodide of antimony, Faraday found that all solid non-conductors, becoming conductors when liquefied by heat, are decomposed.†

Faraday conceived it to be deducible from his experiments that binary compounds composed of elements, one of which goes to the negative and the other to the positive pole, are decomposable by electricity, but not ternary compounds. Thus the simple oxides, chlorides, and iodides are decomposed, while

* Phil. Trans. 1834, p. 81. † Ibid.

many binoxides, perchlorides, and periodides are not acted on. But to this supposed law there are so many exceptions, that it would not be safe to adopt it. Chap. XV.

Mr Faraday found reason to conclude from his experiments, that the following bodies are not decomposed by electricity, and it would seem that they are non-conductors :—

Sulphuric acid
Phosphoric acid
Arsenic acid
Hyponitrous acid
Nitric acid?
Chloride of sulphur
Protochloride of phosphorus
Protochloride of carbon

Periodide of mercury when liquid is a conductor, but is not decomposed by electricity.

Perchloride of mercury when liquid is a conductor, and seems to be decomposed by electricity.

Protoxide of antimony when liquid is a conductor, and is decomposed.

The following bodies also were found by Faraday not decomposable by electricity :—

Nitre	Tartaric acid
Nitrate of ammonia	Tartrates
Sulphurous acid	Benzoates
Hydrofluoric acid	Sugar
Fluorides	Gum
Acetates	

But the following bodies are decomposed by the action of the Voltaic battery :— Substances decomposed.

Muriatic acid	Iodides
Hydriodic acid	Hydrocyanic acid
Chlorides	Cyanodides

Before entering more particularly into the explanation of chemical decompositions by electricity, it will be proper to ex-

Part II.

plain the meaning of some new terms which Mr Faraday has found it convenient to introduce.

Explanation of terms.

Electrolyte, is a body directly decomposable by the electric current. Thus, water is an electrolyte.

Ions, a general name for the elementary constituents of electrolytes. Thus, oxygen and hydrogen are the *ions* of water.

Anode, the surface by which electricity enters, or the *positive* pole.

Cathode, the surface by which electricity leaves the body, or the *negative* pole.

Electrodes, the poles of the battery.

Anions, bodies going to the *anode* or *positive* pole.

Cations, bodies going to the *cathode* or *negative* pole.

Compound bodies may be divided into two great classes, namely, those which are decomposable by the electric current, and those which are not. Of the latter, some are conductors and some non-conductors of Voltaic electricity.

The decomposable bodies do not depend for their decomposability upon the nature of their elements, but upon the proportions in which these elements are combined. These decomposable bodies Faraday distinguishes by the name of *electrolytes*.

The elements into which these electrolytes are divided by the Voltaic actions are *combining* bodies, and have each a definite proportion in which they are always evolved during electrolytic action. They are called *ions*, and divided into *anions* and *cations* according as they appear at the anode or cathode end of the Voltaic battery. The proportions in which they are respectively evolved by electricity are proportional to the atomic weights of each body respectively. Thus hydrogen, oxygen, chlorine, iodine, lead, and tin, are evolved as the numbers 0·125, 1, 4·5, 15·75, 13, 7·25. The following are the laws of the decomposition of these ions as laid down by Mr Faraday:—

Laws of electrical decompositions.

1. An *ion*, not in combination with another, is indifferent, having no tendency to pass to either of the electrodes.

2. If one *ion* be combined in right proportions with another

strongly opposed to it in its ordinary chemical actions, or in other words, if an *anion* be combined with a *cation*, then both travel the one to the *anode*, the other to the *cathode* of the decomposing body. Chap. XV.

3. If an *ion* pass to one of the electrodes, another *ion* must pass at the same time to the other electrode.

4. Every body decomposable by electricity, must consist of two *ions*, the one of which must go to the *cathode* and the other to the *anode*.

5. Only a single electrolyte, composed of the same elementary *ions*, seems to exist. Thus *water* is the only electrolyte composed of *oxygen* and *hydrogen*.

6. A body not decomposable when alone, as boracic acid, is not directly decomposable by the electric current, when in combination.

7. The nature of the substance of which the electrodes are composed, provided it be a conductor, causes no difference in the electro-decomposition, either in kind or degree; but it seriously influences, by secondary action, the state in which the ions finally appear.

8. A substance, which being used as an electrode is capable of combining with the *ion* evolved against it, is also an *ion*, and combines in the quantity represented by its atomic weight.

9. Compound *ions* are not necessarily composed of simple *ions* united in simple atomic proportions. Thus, sulphuric acid is an *ion*, but not an *electrolyte*.

In many instances, the *secondary result* obtained by the action of the evolving *ion* on the substances present in the surrounding liquid, will give the electro-chemical equivalent. Thus in the solution of acetate of lead, the number of atoms of lead reduced is equal to those of hydrogen evolved at the *cathode*: hence, secondary results may occasionally be used as measures of the Voltaic electrical current.

The following table exhibits a list of the principal ions, together with the atomic weights of each, which represents the proportions by weight in which they are respectively evolved:—

Part II.

Table of ions.

I. ANIONS.

Oxygen	1	Chloric acid	9·5
Chlorine	4·5	Phosphoric acid	4·5
Bromine	10·	Carbonic acid	2·7
Iodine	15·75	Boracic acid	3
Fluorine	2·25	Acetic acid	6·3
Cyanogen	3·25	Tartaric acid	8·2
Sulphuric acid	5	Citric acid	7·2
Selenic acid	8	Oxalic acid	4·5
Nitric acid	6·75		

II. CATIONS.

Hydrogen	0·125	Silver	13·5
Potassium	5	Gold	12
Sodium	3	Platinum	12
Lithium	0·75	Palladium	6·75
Barium	8·5	Rhodium	6·75
Strontium	5·5	Iridium	12·25
Calsium	2·5	Ammonia	2·12
Magnesium	1·5	Potash	6
Aluminum	1·25	Soda	4
Iron	3·5	Lithia	1·75
Manganese	3·5	Barytes	9·5
Nickel	3·25	Strontian	6·5
Cobalt	3·25	Lime	3·5
Zinc	4·125	Magnesia	2·5
Cadmium	7	Alumina	2·25
Lead	13	Quinina	20·25
Tin	7·25	Cinchonina	19·75
Copper	4	Morphina	35·5
Bismuth	9	Protoxides	
Mercury	12	Vegetable alkalies.	

It is pretty obvious, (provided the preceding views be correct,) that every atom of matter must be naturally surrounded by the same quantity of electricity, so that in this respect heat and electricity resemble each other.

Electrolytes differ in facility of decomposition.

Electrolytes differ in the facility with which they may be decomposed. Mr Faraday has found the following to be electrolytic in the order in which they are placed, those highest up being decomposed by a current of the lowest intensity:—*

* Phil. Trans. 1834, p. 433.

Iodide of potassium, (solution)
Chloride of silver, (fused)
Protochloride of tin, (fused)
Chloride of lead, (fused)
Iodide of lead, (fused)
Muriatic acid, (solution)
Water acidulated with sulphuric acid.

It is well known, that when a piece of zinc is placed upon the surface of the copper sheathing of the bottom of a ship, the zinc is corroded, but the copper remains quite bright and is not oxydized as it is in ordinary cases. It was shown by Davy, that a little bit of zinc will keep bright a considerable surface of copper. In this case, a decomposition of water takes place, accompanied by the usual disengagement of electricity. The oxygen unites with the zinc, while the hydrogen is evolved over the whole surface of the copper for a considerable way round the zinc. It is this evolution of hydrogen that prevents the copper from being oxydized.*

When we plunge one after another the two ends of the copper wire of a multiplier into nitric acid, electricity is evolved.†

Gold or platinum wire, while of the same temperature, cause no electric current when plunged into nitric acid; but if we heat one end a current is evolved. A current also appears directly if we add a little muriatic acid, so as to cause the acid to act upon the gold or platinum.‡

* Daniell, Phil. Trans. 1836, p. 113.
† Becquerel, Traité de l'Electricité, ii. 74. ‡ Ibid. p. 75.

CHAPTER XVI.

OF THE VOLTAIC PILE.

Electricity by contact of two metals.

If we take a number of thin plates of glass, and cover the under surface of each with a thin coating of zinc, and the upper surface with an equally thin coating of silver or copper, and if we place these discs above each other in a determinate order; namely, the zinc side of each always undermost, and the silver or copper side always uppermost, slight electrical effects will be produced, which increase in intensity as we augment the number of discs of glass. When two of these dics are laid upon each other, the silver coating will be electrified minus, and the zinc coating plus. The undermost side of the second glass disc will, therefore, be in a state of positive excitement: it will therefore expel positive electricity from, and attract negative electricity to, the upper side of the disc which is coated with silver. Suppose, now, another glass disc similarly coated to be applied. The silver side of the second disc is minus before the application of the new disc; the zinc coating, in consequence, will become more positive than the zinc coating of the second disc: hence the upper side of this third disc will be more negative than was that of the upper side of the second disc. It is easy to see how the charge increases in intensity with the number of discs. The apparatus is analogous to the electrophorus or the condenser. The quantity of electricity thus disengaged is extremely small, and it does not become sensible till the discs of glass amount to thirty or forty. The opposite sides of every glass are in different electrical states, and the quantity of disengaged electricity in each increases with their number. If we bring the two sides of each disc in contact, by means of a wire, the equilibrium is

instantly restored, and some time elapses before the discs become again charged.*

Pile by two metals and paper.

If instead of glass, which is a non-conductor of electricity, we employ an imperfect conductor, as a leaf of paper, similar electrical phenomena make their appearance. Suppose we take round pieces of paper of the size of half-a-crown, silvered on the one side, by the application of very thin zinc leaf, and gilt on the other by means of Dutch leaf, and pile them above each other, so that the silvered side of each shall be always undermost, we obtain an apparatus which will exhibit electrical phenomena. The method is to put the paper discs within a glass cylinder, and, at least, a couple of thousand paper discs must be employed, so as to fill the glass cylinder completely from top to bottom. The extremities of the glass cylinder are shut by plates of brass, directly in contact with the paper discs, and provided with metallic buttons. The button at the zinc end of the cylinder is always in the state of positive electricity, while the button at the copper end of the cylinder is in the state of negative electricity. This kind of apparatus was first constructed by De Luc, and afterwards by Zamboni, and they have been constructed in different ways; though, as the theory is the same in all, it seems unnecessary here to enter into details.

The paper in these piles takes the place of the disc of glass in the former pile. Now, as paper is an imperfect conductor, if we discharge the electricity, by bringing the top and bottom of the pile in contact by means of a wire, the charge is very speedily renewed when the wire is withdrawn. Two of these piles are usually placed at a small distance from each other, and so that the positive pole of the one corresponds with the negative pole of the other. A very light and insulated needle suspended in equilibrium upon a very moveable axis between these two poles, oscillates continually between them. The piles discharge their electricity by means of this needle, which is, in consequence, attracted alternately by the one and the other. De Luc placed a small bell between the two piles,

* See Jaeger's experiments with this pile, Gilbert's Annalen, xlix. 64.

Part II.

which was struck by small metal balls suspended between the pole of the piles and the bell. These small balls, being alternately attracted and repelled, kept striking the bell, which thus continued for a long time continually ringing. Sometimes it stopped suddenly; but after a certain interval it began again, and the bell kept ringing as before.

These piles continue to act for several months; but they finally lose their activity, owing to the small quantity of water originally in the paper being at last totally decomposed. Even before they finally become inactive, their intensity varies with the temperature, and the moisture or dryness of the air.

When the paper of which these piles are constructed is made perfectly dry, their intensity is very much diminished, obviously because dry paper is a non-conductor of electricity. The consequence is, that when the electricity is discharged, a considerable time elapses before it accumulates again. But Dr Jaeger found that when the temperature was raised to 104°, or as high as 140°, the pile begins again to act as well as ever.* We must conclude from this, that dry paper, while cold, is a non-conductor of electricity, but that it becomes again a conductor, when heated up to 104° or 140°.†

Volta's pile. If, instead of solid non-conductors, or demi-conductors, we employ liquid bodies which conduct electricity, the energy of the pile is prodigiously increased. A pile constructed in this way, constitutes the pile of Volta, and it must be ranked among the greatest discoveries of modern times. It has not only added greatly to our knowledge of electricity, but has contributed, in a surprising degree, to the extension and perfection of chemical science. If we compare the state of chemistry before the discovery of the Voltaic pile with its present aspect, we cannot but be astonished at the difference, and this difference is in a great measure owing to the discoveries made by means of this new instrument of investigation.

* Gilbert's Annalen der Physik, xlix. 47.

† The reader who is interested in the investigations of these dry piles, as they have been called, may consult Gilbert's Annalen der Physik. vol. xlix., in which both the papers of De Luc and Zamboni, and the investigations of Jaeger, are to be found.

I have already, in a preceding chapter, given an account of the controversy between Galvani and Volta, which led to the discovery of the *pile*. As it was originally constructed by Volta, it consisted of a number of round pieces of zinc, and silver, or copper, each about the size of half-a-crown. There were as many pieces of pasteboard, as there were of zinc, of the same round shape, but a little smaller size than the metal discs. These pieces of pasteboard were soaked in an aqueous solution of common salt, which is a better conductor of electricity than pure water. These pieces of metal and moist card were piled upon each other in the following order:— Zinc, silver, moist card; zinc, silver, moist card, &c., always observing the same order, till all the pieces be piled upon each other. The card should not be so much soaked with liquid, that the weight of the plates laid above it will be sufficient to squeeze it out, and cause it to run down the pile. Because in that case, it would insinuate itself between the zinc and silver discs, which has a surprising effect in diminishing the efficacy of the pile. In such a pile, the uppermost plate is silver, and the undermost is zinc. The intensity of the electrical phenomena increases with the number of pairs of plates; but they become sensible when the pairs of plates amount to about a dozen. The zinc extremity of such a pile is charged positively, and the silver or copper extremity negatively. If these two extremities be brought into contact, by means of a conducting substance, a metallic wire for example, a current of electricity sets through the wire, and continues to pass through it as long as the pile retains its activity. Electric sparks may be taken, and a Leyden jar may be charged by such a pile, precisely as by an electrical machine, though only to a low degree of intensity. If we moisten the finger of each hand, and apply them, thus wet, to the two extremities of the pile, at the instant of contact we feel a shock, the intensity of which increases with the number of pairs of plates of which the pile is composed. As long as the fingers are kept in contact with the poles of the pile, we feel no sensation of pain, unless a portion of the cuticle be rubbed off, or any part of the finger wounded, in which case we become sensible of a burning sensa-

Part II. tion in the wounded part, which becomes more and more painful according to the number of plates, and the degree of activity of the pile. If we touch the negative end of the pile with the wet finger, and bring a platinum or gold wire from the positive end in contact with the tongue, we become sensible of a strong acid taste. If the wire from the negative end be placed in contact with the tongue, while we touch the positive pole with a wet finger, we become sensible of a strong burning or alkaline taste. In the same manner, we become sensible of light, when the electric current is made to pass through the eye, or to approach the nerves distributed to the eye.

Such were the phenomena observed by Volta, the inventor of the pile, and described by him in his first paper on the subject.* It will be proper, before proceeding farther, to explain the various modifications which Volta's pile underwent in the hands of chemical experimenters.

Improved by Cruikshanks. The first improvement was made by Mr Cruikshanks of Woolwich. The object of it was to facilitate the construction of the pile. He employed square plates of zinc and copper, each about four inches long, and as much in breadth. These plates were soldered together, two and two; and cemented in a trough of wood, by means of pitch, or any non-conducting substance with which the inner side of the trough was covered. These soldered plates of zinc and copper were placed all in the same way in the trough; that is to say, the zinc sides were all turned towards one end of the trough, and the copper sides towards the other end. These plates were cemented into the trough, at the distance of about a quarter of an inch from each other, thus dividing the trough into as many separate cells as there were pairs of plates. To make the trough fit for action, nothing more was necessary than to fill the cells with the liquid destined to act as a conductor. It is evident that such a trough would act the part of Volta's pile; and that the end of it terminated by the zinc plate would be positive, while that terminated by the copper plate would be negative.

* Phil. Trans. 1800, p. 403.

The next modification was to substitute wooden or stone-ware troughs, divided each into ten cells. The zinc and copper plates, instead of being soldered together along their whole surface, had each a ribbon of copper, proceeding from the top of the copper plate, and soldered to the top of the zinc plate. This ribbon was of such a length as just to pass over the division between two adjacent cells, so that the zinc plate could be placed in one cell and the copper in the next adjacent. In this way, the plates were arranged so as to fill the trough, (as has been explained in a preceding chapter,) and the battery was increased *ad libitum*, by placing any number of troughs side by side, and connecting them with each other.

By Wollaston.

It was observed by Dr Wollaston, that the electricity evolved was proportional to the surface of copper opposite to the surface of zinc. The zinc plate is the one that is corroded and destroyed by the action of the dilute acid in the cells; but it is acted upon on both sides, while in the original construction of the battery, only one side of the zinc is opposed by copper. This causes a useless waste of the zinc plates without a corresponding evolution of electricity. To remedy this defect, he suggested the propriety of surrounding the zinc plate with copper, taking care that the two metals did not come in contact, except where the copper ribbon is soldered to the zinc. This new modification was found considerably to increase the energy of the battery.

Morichini has shown that the energy of the apparatus increases in proportion as the surface of the electro-negative metal is extended. This metal may, he says, be sextupled with always an increasing energy; beyond that point, the rate at which the energy increases is slower.*

Hare's rolled plates.

Dr Hare, professor of chemistry at Philadelphia, has contrived a new modification of the galvanic apparatus. He takes two plates, one of copper and the other of zinc, and placing a disc of leather between the two, he rolls them up in the form of a spiral. The leather disc is now removed, and the two metals

* Berzelius, Traité de Chimie, i. 138.

Part II. are prevented from touching each other, by interposing slips of wood. Each of these plates is made to communicate with a plate of a different kind of metal, the zinc with copper, and the copper with zinc, precisely in the way already described when giving an account of the troughs. A number of these double spirals are fixed upon a piece of wood in the same way as has been just explained. These cylinders are now introduced, each into a cylindrical glass vessel. This method of construction is attended with several important advantages. By this contrivance plates of a very large size may be introduced into a very small vessel; so that the expenditure of liquid is the least possible. Besides, the greatest part of the two sides of each plate is active, being placed opposite to a face of the other metal.

Deflagrator.

Dr Hare has constructed another apparatus, which he calls a *deflagrator*, and which appears to possess very great power. He takes a plate of zinc three or four inches square, and encloses it in a case of copper, distant from it about a line, and touching it nowhere. Any number of these plates thus enclosed in copper is attached to a horizontal piece of wood, and fixed immovably that there may be no risk of the plate of zinc touching the copper case in which it is enclosed. The zinc plate at the first of these is united at the top to the copper case of the next zinc plate; and this is continued through the whole. These copper cases are placed at a very small distance from each other, and between each pair is introduced a piece of card dipped in linseed-oil varnish, and half dry. They are then compressed so as to adhere so closely to each other, that no water can insinuate itself between them. Things being thus disposed, the apparatus is plunged into a trough containing the liquid, and not divided into cells; the varnished card answering all the purposes of the diaphragms in the porcelain troughs. Four such pieces of apparatus, containing each fifty plates of zinc, surrounded each by its copper case, when plunged into their proper troughs, produce very powerful effects.

The apparatus employed by Œrsted, and of the efficacy of which he speaks in high terms, approaches very nearly to this

last one of Hare. Indeed the theoretical construction of both is the same.

The improvements in the Voltaic battery suggested by Dr Hare have been fully confirmed by Mr Faraday.* It will be proper, in this place, to give a concise account of the most important of these improvements.

In the Voltaic battery, the chemical forces which, during their activity, give power to the instrument, may be divided into two portions. The first of these is exerted locally, while the second is transferred round the circle. The latter constitutes the electrical current, while the former is altogether lost or wasted. The ratio between these two portions may vary exceedingly. When the battery is not closed, all the action is local. When amalgamated zinc plates are used, in the way first suggested by Mr Kemp,† all the chemical power circulates and becomes electricity.

Importance of amalgamated zinc.

Amalgamated zinc even though impure does not sensibly decompose the water of dilute sulphuric acid, but still has such an affinity for the oxygen, that the moment a metal, as copper, which has but little affinity, touches it in the acid, a powerful and abundant electric current is produced. Faraday conceives that the mercury acts by bringing the surface, in consequence of its fluidity, into one uniform condition, and preventing those differences in character between one spot and another, which are necessary for producing the minute Voltaic circuits described by Mr Faraday.‡ If any difference does exist at the first moment, with regard to the proportion of zinc and mercury at one spot of the surface, as compared with that of another; that spot which has the least mercury being first acted on, is soon, by the solution of the zinc, placed in the same condition with the other parts, and the whole plate rendered uniform on the surface: hence, one part cannot act as a discharger to another, and the full equivalent of electricity is obtained for the oxydation of a certain quantity of zinc. A battery constructed with the zinc so prepared, and charged

* Phil. Trans. 1835, p. 263.

† Jameson's Edinburgh Journal, October, 1828.

‡ Phil. Trans. 1834, p. 457.

Part II. with dilute sulphuric acid, is active only while the electrodes are connected, and ceases to be acted on by the acid the moment the communication is broken.

Mr Faraday constructed a small battery of ten pairs of plates, consisting of amalgamated zinc and platinum connected together by being soldered to platinum wires. The apparatus had the form of the *couronne des tasses*. The liquid used, was dilute sulphuric acid, of the specific gravity 1·25. No action took place upon the metals, except when the electrodes were in communication, and then the action on the zinc was only proportional to the decomposition of water in the experimental cell; that is to say, for every 0·125 grains of hydrogen gas evolved, 4·125 grains of zinc were dissolved by the sulphuric acid in each cell. The advantage of such a battery is, that it will remain much longer active than an ordinary battery, and the zinc plate will be much less corroded in a given time.

We can always determine the quantity of zinc which goes to waste in a Voltaic battery, by comparing the quantity of hydrogen evolved in the experimental cell, with the quantity of zinc dissolved in any of the cells of the battery. If 4·125 of zinc be dissolved for every 0·125 of hydrogen evolved, there is no waste. If 8·25 zinc be dissolved for every 0·125 hydrogen, then half the zinc is dissolved without contributing any thing to the electric current: and so with every other proportion.

If a Voltaic battery were constructed of zinc and platinum, the latter metal surrounding the former, as in the double copper arrangement, and if the whole were excited by dilute sulphuric acid, no insulating divisions of glass, porcelain, or air, would be required between the contiguous platinum surfaces. And, provided these did not touch metallically, the same acid which, between the zinc and platinum would excite the battery to powerful action, would, between the two surfaces of platinum, produce no discharge of the electricity, nor cause any diminutions of the power of the trough. This is a necessary consequence of the resistance to the current which Faraday has shown occurs at the place of decomposition.*

* Phil. Trans. 1834, p. 460.

If the metal surrounding the zinc be copper, and if the acid be nitro-sulphuric, then a slight discharge between the two contiguous coppers does take place, provided there be no other channel open by which the forces may circulate; but when such a channel is permitted, the return discharge is exceedingly diminished. Ch. XVI.

Improved Voltaic battery

These principles, no doubt, guided Dr Hare in the construction of his trough;* at least they led Faraday to adopt an instrument of precisely the same kind.† The zinc plates were cut from rolled metal, and when soldered to the copper plates, had the form delineated in fig. 1. *z* representing the

Fig. 1.

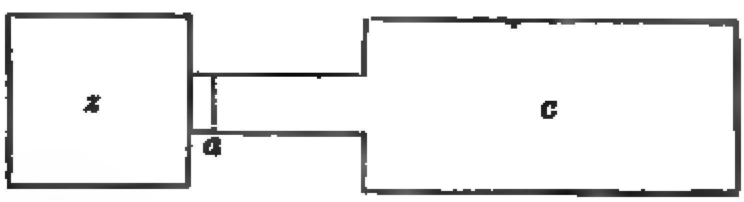


Fig. 2.

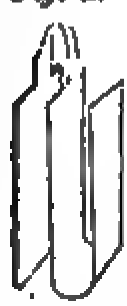
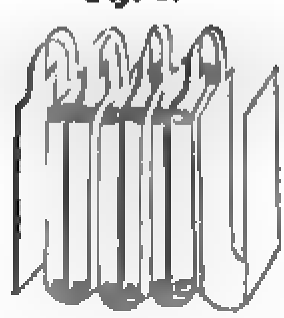


Fig. 3.



zinc, and *c* the copper plate, soldered together at *a*. They were bent over a gauge into the form fig. 2, and when packed into the wooden box, constructed to receive them, were arranged as in fig. 3; little plugs of cork being used to prevent the zinc plates from touching the copper plates, and a single or double thickness of cartridge paper being interposed between the contiguous surfaces of copper, to prevent them from coming in contact. The advantage of this construction is the facility with which the battery can be packed and unpacked, the very small room which it occupies, and the small quantity of dilute acid required to bring it into action.

Dr Hare contrived to make the plates turn on a pivot, so as to be taken at once out of the trough by simply turning

* Phil. Mag. lxiii. 241; and Silliman's Jour. vii. 347.

† Phil. Trans. 1835, p. 264.

Part II.

them round. These pivots give fixed terminations, which enable us to arrange an apparatus to be used with the battery, before we bring the battery into action.

The trough is put into readiness for use in an instant, a single jug of dilute acid being sufficient to charge a hundred pairs of four inch plates. On making the trough pass through a quarter of a revolution it becomes active, and the great advantages obtained, of procuring for the experiment the effect of the first contact of the zinc and acid, which is twice, and sometimes even thrice, that which the battery can produce in a minute or two after.

When the experiment is completed, the acid can be at once poured from between the plates, so that the battery is never left to waste during an unconnected state of its extremities. This will constitute a considerable saving both of the zinc and the acid. In consequence of this saving, thinner plates of zinc may be used, and Mr Faraday has found rolled zinc to be superior to cast zinc in action, owing probably to its greater purity, or perhaps to the greater smoothness and evenness of the surface. By using a due mixture of nitric and sulphuric acid for the charge, no gas is evolved from the troughs; so that a battery of several hundred pairs of plates may, without inconvenience, be close to the experimenter. If, during a series of experiments, the acid becomes exhausted, it can be withdrawn and replaced by other acid with the utmost facility, and the plates may be easily washed after the experiment is concluded.

Mr Faraday has made some practical observations respecting the construction and use of the Voltaic battery, the result of a great deal of practice and acute observation. They deserve, on that account, to be stated in this place.*

Effect of acid of various strengths.

1. *Nature and strength of the acid.*—Mr Faraday charged a battery of forty pairs of three inch plates, with a mixture of 200 water, and 9 of sulphuric acid of commerce. Each plate lost 4·66 atoms of zinc for every atom of water decomposed: so that the whole battery lost 186·4 atoms of zinc, for every

* Phil. Trans. 1835, p. 268.

atom of water decomposed. Being charged with a mixture of 200 water and 16 of muriatic acid,* each plate lost 3·8 atoms, or the whole battery 152 atoms of zinc for every atom of water decomposed. Being charged with a mixture of 200 water and 8 nitric acid,† each plate of zinc lost 1·85, or the whole battery 74·16 atoms for every atom of water decomposed. The sulphuric and muriatic acids evolved much hydrogen gas at the plates in the trough; but the nitric acid occasioned the evolution of no gas whatever. From these experiments, it is obvious that nitric acid is the best. Mr Faraday thinks that it owes this superiority to its favouring the electrolyzation of the liquid in the cells of the trough, and consequently favouring the transmission of electricity, and of course the production of transferable power.

The addition of nitric acid improves both sulphuric and muriatic acids. When the same trough was charged with a mixture of 200 water, 9 sulphuric acid, and 4 nitric acid, each plate of zinc lost 2·786, and the whole battery 111·5 atoms for every atom of water decomposed. When the charge was 200 water, 9 sulphuric acid, and 8 nitric acid, the loss per plate was 2·26 atoms, or for the whole battery 90·4 atoms for every atom of water decomposed. When the charge was 200 water, 16 muriatic acid, and 6 nitric acid, the loss per plate was 2·11, and of the whole battery 84·4 atoms for each atom of water. We see here the advantage of adding nitric acid to sulphuric. Mr Faraday's usual charge is 200 water, 4½ sulphuric acid, and 4 nitric acid.

It would appear from Mr Faraday's experiments, that the increase of the strength of the acid does not add much to the electrolytic effects. When the trough was charged with a mixture of 200 water and 8 nitric acid, each plate lost 1·854 atoms of zinc for the decomposition of 1 atom of water. When the charge was 200 water and 16 nitric acid, the loss per plate was 1·82 atoms zinc; when it was 200 water and 32 nitric acid, the loss was 2·1 atoms per plate. When a charge, consisting of 200 water, 4½ sulphuric acid, and 4 nitric acid, was used, each plate

* One cubic inch of this acid dissolved 108 grains of marble.

† One cubic inch of this acid dissolved 150 grains of marble.

Part II. lost 2·16 atoms; with a charge of 200 water, 9 sulphuric acid, and 8 nitric acid, the loss was 2·26 atoms.

No copper is dissolved during the regular action of the Voltaic trough. Much ammonia is formed in the cells when nitric acid either pure or mixed with sulphuric is used.

2. The uniformity of the charge in all the different cells of the troughs is of great consequence to the regular action of the battery.

Zinc should be pure.

3. *The zinc.*—If pure zinc could be obtained, it would be very advantageous in the construction of the Voltaic apparatus. Most zincs when put into dilute sulphuric acid, leave more or less of an insoluble matter upon the surface in form of a crust. It is a mixture of copper, lead, zinc, iron, cadmium, &c. Such particles by discharging part of the transferable power, diminish the effect of the battery. No gas ought to rise from the zinc plates, the more gas is generated upon them, the greater is the local action and the less the transferable force. The investing crust upon the surface of the zinc plates is also inconvenient, by preventing the displacement and renewal of the charge upon the surface of the zinc. The more slowly the zinc dissolves in the dilute acid so much the better. Zinc containing much copper should be particularly avoided; Mr Faraday found rolled Liege or Mosselman's zinc the best.

After use, the plates of a battery should be cleaned from the metallic powder upon their surfaces. If a few foul plates are mingled with many clean ones they make the actions in the different cells irregular, the transferable power is diminished, while the local and wasted power is increased.

Voltaic batteries are much more powerful when the plates are new than after they have been used two or three times. A trough of twenty pairs of four inch plates, charged with 200 water, 4½ sulphuric acid, and 4 nitric acid, lost the first time it was used 2·32 atoms of zinc per plate. When used after the fourth time with the same charge, the loss was from 3·26 to 4·47 atoms per plate. The average being 3·7 atoms the first time the forty pairs of plates trough was used, the loss at each plate was only 1·65 atoms; but afterwards it became 2·16,

2·17, 2·52 atoms. The first time twenty pairs of four inch plates in a porcelain trough were used, the loss per plate was 3·7 atoms; but afterwards the loss became 5·25, 5·36, 5·9 atoms, yet in all these cases the zincs had been well cleaned from adhering copper, &c., before each trial of power.

With rolled zinc the fall of force soon ceased to proceed any farther; but with cast zinc plates belonging to the porcelain troughs, it appeared to continue until at last with a given charge each plate lost above twice as much zinc for a given amount of action as at first.

Zinc and copper should be near each other.

4. *Vicinity of the copper and zinc.*—It has been long known that the action of the battery is much promoted by bringing the copper and zinc surfaces as near to each other as possible. Mr Faraday has found that the sum of the transferable power compared with the whole amount of the chemical action on the plates is much increased by this proximity. The reason of this is evident. Whatever tends to retard the circulation of the transferable force, diminishes that force while it increases the local action. The liquid in the cells possesses this retarding power, and therefore acts injuriously according to the quantity of it between the zinc and copper plates: hence a trough in which the plates are only half the distance asunder at which they are placed in another, will produce more transferable and less local effect than the latter.

The superiority of double coppers over single plates depends in part upon diminishing the resistance offered by the electrolyte between the metals; for with double coppers the sectional area of the interposed acid becomes nearly double that with single coppers, and of course it transmits the electricity more freely. Double coppers are effective chiefly because they virtually double the acting surface of the zinc; for in a trough with single copper plates and the usual construction of cells, that surface of zinc which is not opposed to a copper surface is thrown almost entirely out of the Voltaic action, yet the acid continues to act upon it, and to dissolve it without producing any beneficial effect. When by doubling the copper, that metal is opposed to the second surface of the zinc plate, a great part of the action upon the latter is con-

Part II.

verted into transferable force, and thus the power of the trough is much increased.

5. *First immersion of the plates.*—Mr Faraday ascribes the great effect produced at the first immersion of the plates to the unchanged condition of the acid in contact with the zinc plate. As the acid becomes neutralized, its exciting power is proportionably diminished. Dr Hare's form of trough secures much advantage of this kind, by mingling the liquid and bringing what may be considered as a fresh surface of acid against the plates every time it is used immediately after rest.

Effect of number of plates.

6. *Number of plates.*—The most advantageous number of the plates in a battery used for chemical decomposition depends upon the resistance to be overcome at the place of action; but whatever that resistance may be, there is a certain number of plates which is most economical. Ten pairs of four inch plates in a porcelain trough of the ordinary construction, acting upon dilute sulphuric acid of specific gravity 1·314, gave an average consumption of 5·4 atoms of zinc per plate, or 54 atoms on the whole. Twenty pairs of the same plates with the same acid, give a consumption of 5·5 atoms per plate, or 110 atoms on the whole; while forty pairs of the same plates with the same acid gave 3·54 atoms per plate, or 141·6 atoms on the whole. Thus the consumption of zinc arranged in twenty plates was more advantageous than if arranged either as ten or as forty. Ten pairs of 4 inch plates lost each 6·76 atoms, or 67·6 atoms of zinc on the whole in effecting decomposition: while twenty pairs of the same plates, excited by the same acid, lost 3·7 atoms each, or 74 atoms on the whole. In other comparative experiments made by Mr Faraday, ten pairs of 3 inch plates lost 3·725 atoms, or 37·25 on the whole; while twenty pairs lost 2·53 atoms each; or 50·6 in all, and forty pairs lost 2·21 atoms each, or 88·4 atoms in all. Thus it appears, that with acid of the strength above indicated, twenty pairs of plates are more economical than either ten or forty pairs.

But when a weaker acid or worse conductor is used, the number of plates which are most advantageous is increased: on the other hand, by using a stronger acid or a better conductor, the number of plates may be reduced even to a single

pair, as, for instance, when a thick wire is employed to complete the circuit. The reason of this is, that each successive plate in the Voltaic apparatus does not add any thing to the *quantity* of transferable power or electricity which the first plate can set in motion, provided a good conductor be present; but tends only to increase the intensity of that quantity, or to make it more able to overcome the obstruction of bad conductors.

Of large plates.

7. *Large or small plates.*—The advantageous use of large or small plates for electrolyzations depends upon the facility with which the transferable electricity can pass. If in a particular case the most effectual number of plates is known, the addition of more zinc would be most advantageously made by increasing the size of the plates and not their number. Large increase of the size of the plates raises in a small degree the most favourable number. Large and small plates should not be used together in the same battery. The small ones occasion a loss of power of the large ones, unless they be excited by an acid proportionally more powerful; for with the same acid they cannot transmit the same portion of electricity in a given time as is evolved from the larger plates.

Effect of various conductors.

8. *Simultaneous decompositions.*—When the number of plates in a battery much surpasses the most favourable proportion, two or more decompositions may be effected at the same time with advantage. Thus in Mr Faraday's trough of forty pairs, one Volta's electrometer produced 22·8 cubic inches of gas in a given time. Being recharged exactly in the same manner, the produce in each of two Volta electrometers was 21 cubic inches. In the first experiment the whole consumption of zinc was 88·4 atoms, and in the second only 48·28 atoms, for the whole water decomposed in both electrometers; but when twenty pairs of 4 inch plates were tried in the same manner, the results were of an opposite nature. With one electrometer 52 cubic inches of gas were obtained; with two only 14·6 cubic inches in each. The consumption of metal in the first case was 74, and in the second 97 atoms.

That the *transferring* or *conducting* power of an electrolyte which is to be decomposed should be rendered as good as pos-

Part II. sible is evident. With a perfectly good conductor and a good battery, nearly *all* the electricity is passed even with a single pair of plates. With an interposed non-conductor, none of the chemical power becomes transferable. With an imperfect conductor, *more* or *less* of the chemical power becomes transferable according to circumstances; namely, actual increase or improvement of the conducting power, enlargement of the electrolytes, approximation of the electrodes, and increased intensity of the passing current.

The introduction of common spring water in place of one of the Volta eudiometers used with twenty pairs of 4 inch plates, caused such obstruction as not to allow one-fifteenth of the transferable force to pass which would have circulated without it. Thus $\frac{14}{15}$ths of the available force of the battery were destroyed, yet the platinum electrodes in the water were three inches long, nearly an inch wide, and not a quarter of an inch apart.

When an ordinary Voltaic battery is in action, its very activity produces certain effects which cause a serious diminution of its power. This makes it a very inconstant instrument as to the quantity of effect which it is capable of producing. When a battery is in action, the oxide of zinc formed combines with the acid employed to excite the actions; this occasions the formation of a film of solution of sulphate, (or nitrate or chloride,) of zinc in contact with the zinc plate. The acid being neutralized or nearly so in this film, the oxydizement of the zinc cannot go on with the same facility as before, and the chemical action being thus interrupted, the Voltaic action diminishes with it. This film of liquid is not easily displaced, especially if the surface of the zinc be rough and irregular.

A second cause of diminution in the force of the Voltaic battery, is the extraordinary state of the surfaces of metals in consequence of which reverse currents are produced after the battery has been some time in action. It was first noticed by Ritter, and afterwards experimented on by Marianini and A. de la Rive.

Weak and exhausted charges should never be used at the same time with strong and fresh ones in the different cells of

Ch. XVI.

a trough, or the different troughs of a battery. The fluid in all the cells should be alike, otherwise the plates in the weaker cells, in place of assisting, retard the passage of the electricity, generated in and transmitted across the stronger cells. Each zinc plate so situated has to be assisted in decomposing power before the whole current can pass between it and the liquid. If in a battery of fifty pairs of plates, ten of the cells contain a weaker charge than the others, then, in fact, ten decomposing plates are opposed to the transit of the current of forty pairs of generating plates; so that if the ten pairs of plates were removed, the remaining forty pairs would be much more powerful than the whole fifty.

Associations of strong and weak pairs of plates should be avoided. Faraday found that a pair of copper and platinum plates, arranged in accordance with a pair of zinc and platinum plates, in dilute sulphuric acid, stopped the whole action of the latter.

The reversal, by accident or otherwise, of the plates in a battery, has an exceedingly injurious effect. It is not merely the counteraction of the current which the reversed plates produce, but their effects also in retarding as indifferent plates, and requiring decomposition to be effected on their surface, in accordance with the course of the current before the latter can pass.*

From these observations it is obvious how impossible it is to have a Voltaic battery which will act for a considerable length of time in a constant and uniform manner; yet, for many important objects of investigation, such an apparatus is most important. The scientific world, therefore, lies under great obligations to Professor Daniell for his suggesting a method of constructing a battery which is constant and uniform in its action for a considerable time.†

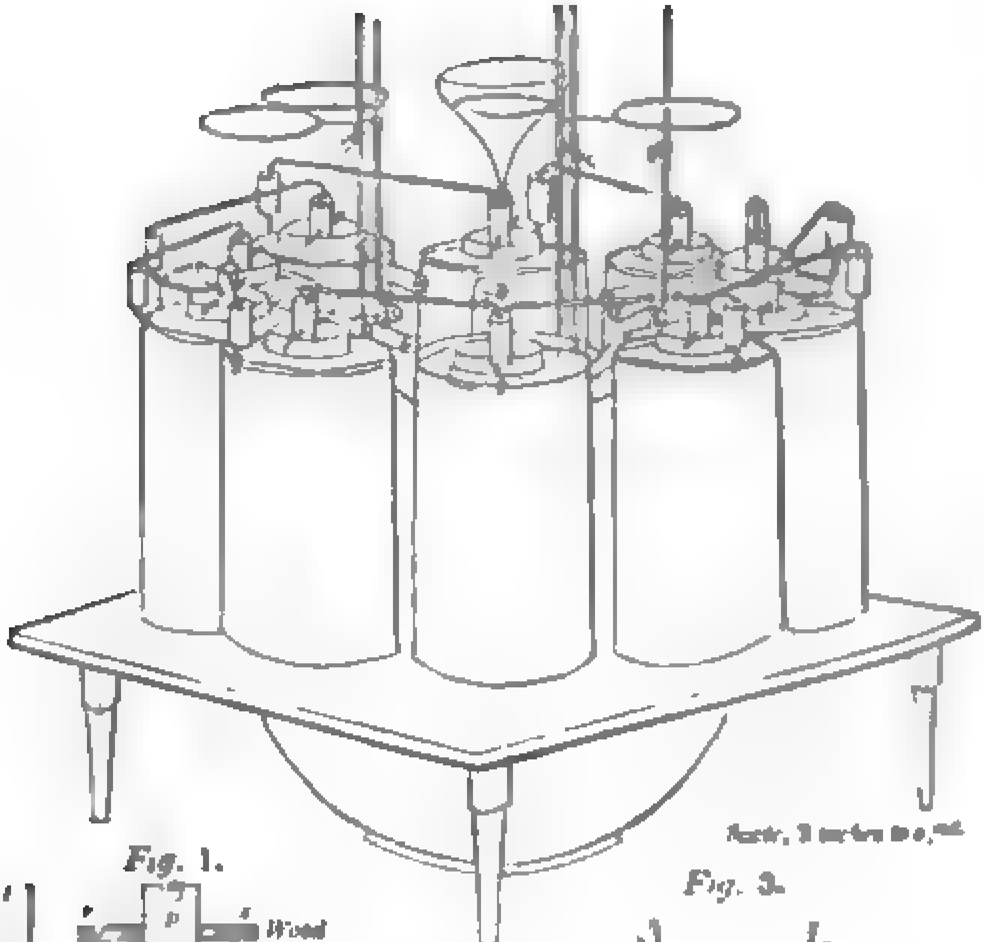
Daniell's constant battery.

His constant battery consists of ten pairs of plates arranged circularly in a kind of *couronne des tasses*. Fig. 1. represents a section of one of the cells, ten of which are shown in connexion in fig. 2. *a b c d* is a cylinder of copper six inches

* Faraday, Phil. Trans. 1834, p. 465. † Phil. Trans. 1836, p. 117.

Part II. high, and three and a half inches wide. It is open at the to (*a b*); but closed at the bottom, except a collar (*e f*), intend

Fig. 2.



Scale, 3 inches to a foot.

Fig. 1.

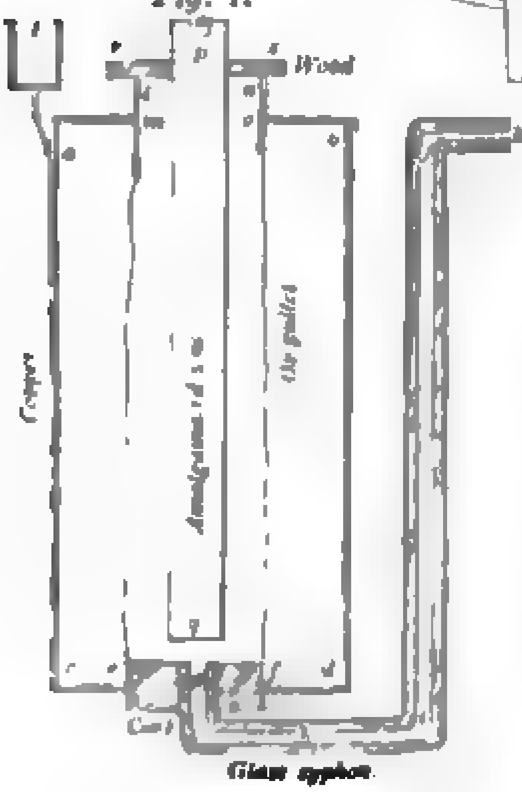
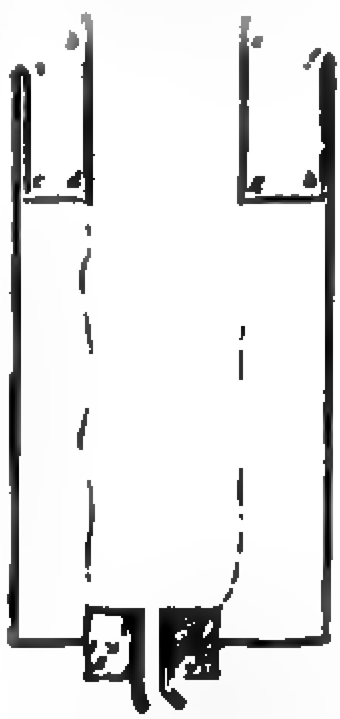


Fig. 3.



Scale, 6 inches to a foot.

for the reception of a cork, into which a glass syphon tube (*g h i j k*) is fitted. On the top (*a b*) a copper collar, corre-

with the one at the bottom, rests by two horizontal
eviously to the fixing of the glass syphon tube in its
embranous tube, formed of a part of the gullet of an
wn through the lower collar (*e f*), and fastened with
ie upper (*l m n o*); and, when tightly fixed by the
w, forming an internal cavity to the cell communi-
h the syphon tube in such a way as that when filled
iquid to the level (*m o*), any addition causes it to
t the aperture (*k*). In this state, for any number of
wed to fall into the top of the cavity, an equal num-
scharged from the bottom; *p q* is a rod of cast zinc,
ted with mercury, six inches long, and half an inch
r, supported on the rim of the upper collar by a
od (*r s*), passing through a hole drilled in its upper
t is a small cup for the reception of mercury, by
l the cavity (*u*) at the top of the zinc rod, various
s of the copper and zinc of the different cells may
by means of wires proceeding from one to the other.
2. ten cells are represented as connected in a single
zinc of one with the copper of the next. They
n a small table in a circle, with the apertures of the
es turned inwards, surrounding a large funnel com-
g with a basin underneath for the reception of any
ch may overflow. A smaller funnel is supported
nternal cavity of each cell by a ring sliding upon
ass placed between each pair of cells. One of these
wn in the figure to avoid crowding.

battery the surface of one of the amalgamated zinc
s than ten square inches, while the internal surface
per cylinder opposed to it is nearly 72 square inches.
cipal object in view is to remove out of the circuit
of zinc, which has been found injurious to the action
mon battery. This is accomplished by the suspen-
e rod in the interior membranous cell, into which
ulated water is allowed slowly to drop from the
pended over it, and the aperture of which is adjusted
rpose; while the heavier solution of the oxide is with-
m the bottom at an equal rate by the syphon tube.

Part II. A second object was to absorb the hydrogen evolved up the copper without the precipitation of any substance whi might deteriorate the latter. This was attained by chargi the exterior space surrounding the membrane with a saturat solution of sulphate of copper instead of dilute acid. Up completing the circuit, the current passed freely through th solution. No hydrogen made its appearance upon the co ducting plate; but a beautiful pink coating of pure copp was precipitated upon it, and thus perpetually renewed i surface.

When the whole battery was properly arranged, and charg in this manner, no evolution of gas took place from the gen rating or conducting plates, either before or after the co nexions were complete. But when a Voltameter was includ in the circuit, the action was found to be very energetic. was also much more steady and permanent than the comm battery; but still there was a gradual, though very slo decline, which Mr Daniell traced to the weakening of th saline solution by the precipitation of the copper, and th consequent decline of its conducting power.

To obviate this defect, he suspended some sulphate o copper in small muslin bags, which just dipped below the sur face of the solution in the cylinders; which gradually dissolv ing as the precipitation proceeded, kept it in a state o saturation. This expedient answered the purpose so wel that the current was perfectly steady for six hours together.

Fig. 3. represents a section of this additional arrangement (*a c h f*) is the collander with its central collar (*b d g e*), which rests by a small ledge upon the rim of the cylinder: the membrane is drawn through the collar, and turning over its edge, is then fastened by twine.

After this alteration, the effective length of the zinc rods exposed to the action of the acid was no more than 4½ inches. The specific gravity of the solution of sulphate of copper was 1·1985, and that of the standard sulphuric acid was 1·0273.

With this charge, after the circuit had been completed for ten minutes, the mean quantity of mixed gases, taken at intervals of five minutes for two hours, was 2·1 cubic inches.

The results never varied more than 0·1 cubic inch from one another. The battery was then left in connexion, without the Voltameter, for two hours, and again examined for three intervals of five minutes. The mean quantity of gases evolved was found the same as before. It was again left in connexion for two hours, and re-examined with the same result. Ch. XVI.

On adding nitric acid to the solution of sulphate of copper, Mr Daniell found that an injurious effect was produced: the mean quantity of gas in five minutes being lowered to 1·1 cubic inch. At this rate of action, however, the battery remained steady for six hours. Mr Daniell then restored the sulphate of copper solution in the exterior cavity of the cells to its original state, and doubled the strength of the dilute acid in the interior portion, adding for this 4½ measures of strong sulphuric acid of commerce to 100 measures of the original weak acid. The specific gravity was now 1·0562; and the mean quantity of gas evolved in the Voltameter was increased to 3·8 cubic inches in five minutes. Removing the meter, and continuing the connexion of the circuit for four hours, and then replacing the meter, the battery was found to be still acting at the same rate. The addition of nitric acid to the solution of sulphate of copper, again reduced the rate to 2·1 cubic inches.

Mr Daniell now added to the sulphuric acid solution an equal volume of nitric acid, and restored the neutral sulphate of copper in the exterior division. At the first impression an increased effect seemed to be produced, and the action for the first quarter of an hour was as high as 4·2 inches of gas in five minutes, but it ultimately settled down and remained at the former amount of 3·8 inches.

Mr Daniell found that the number of atoms of water decomposed by this battery was very nearly equal to the number of atoms of zinc dissolved; so that very little indeed of the action of the battery was local. The whole electricity evolved (very nearly) was transferred in the circuit.

On mixing the same quantity of sulphuric acid as existed in the interior division with the sulphate of copper solution, the battery was made more powerful, yielding 13 cubic inches

Part II. of gas per five minutes, instead of 11 inches. When the t perature of the liquid augmented to 110°, the gas extrica in five minutes was 22 inches. It increased with the t perature, and at 212° was more than doubled.*

Dr Bird's battery. It may be worth while to notice here an ingenious mo cation of Mr Daniell's battery, contrived by Dr Gold Bird.† It is capable of furnishing a constant current of v weak intensity for several weeks, and is therefore very c venient for certain experiments. A glass cylinder, 1·5 i in diameter, and 4 inches in length, is closed at one end means of a plug of plaster of Paris, 0·7 inch in thickn This cylinder is fixed, by means of corks, inside a cylindr glass vessel, about 8 inches deep, and 2 inches in diame A piece of sheet copper, 6 inches long, and 3 inches w (having a copper conducting wire soldered to it) is loo coiled up and placed in the small cylinder with the plaster tom; a piece of sheet zinc of equal size is also loosely co up and placed in the larger external cylinder (being furnis like the copper plate with a conducting wire). The la cylindrical glass being nearly filled with weak brine, and smaller with a saturated solution of sulphate of copper, two fluids being prevented from mixing by the plaster of P diaphragm, the apparatus is complete; and, if care be ta that the fluids in the two cylinders are at the same level, continue to afford a continuous current of electricity for s weeks, the sulphate of copper being very slowly decompo So feeble is the current evolved by an apparatus of this ki that on connecting the two conducting wires with a comm electrometer (having but one needle suspended on a pivot deviation only to 10° or 12° takes place. With Nob Galvanometer (which is much more delicate), a deviation 90° immediately ensues. After this apparatus had been in action for some weeks, chloride of zinc was found in external cylinder, and beautiful crystals of metallic copp frequently mixed with crystals of red oxide of copper, togeth with large crystals of sulphate of soda, were found adheri

* Phil. Trans. 1837, p. 141. † Ibid. p. 39.

to the copper plate in the smaller cylinder, especially on that part where it touched the plaster diaphragm.

Effect of various additions in water.

It has been already observed that pure water is a very bad conductor of electricity. It therefore acts imperfectly as an electrolyte; but it has been long known that the addition of certain substances greatly augments its conducting power. This is the case with sulphuric, phosphoric, oxalic, and nitric acids; but tartaric and citric acids give but little power to water. Acetic and boracic acid produce hardly any effect. Ammonia produces no effect, but its carbonate does. The caustic alkalies and their carbonates produce a fair effect. Sulphate of soda, nitre, and many soluble salts, produce much effect. Cyanodide of mercury, and corrosive sublimate, produce no effect; nor does iodine, gum, or sugar. M. de la Rive has shown that sulphurous acid, bromine, and iodine, produce a considerable effect when added to water, though they are not when alone subject to, or a conductor of, the powers of a Voltaic battery. The chloride of arsenic produces the same effect. In many instances the added substance is acted upon either directly or indirectly, but this is not the case in all.*

Faraday has shown that shell lac becomes a conductor by absorbing ammonia, or muriatic acid; yet both of these substances while gaseous are non-conductors. So is a strong aqueous solution of ammonia.†

It is very remarkable that most electrolytes become non-conductors when they assume the solid state. Metals, on the contrary, continue conductors both when solid and liquid.

Secondary decompositions

It may be worth while to notice here some of the secondary decompositions that take place when Voltaic electricity is made to act on certain liquids.

When a solution of sulphate of ammonia and ammonia is acted on, *azote* is evolved at the positive pole, and *hydrogen* at the negative. Here water is decomposed, and the oxygen at the positive pole decomposes ammonia, setting azotic gas free.‡

* Faraday, Phil. Trans. 1838, p. 94. † Ibid. ‡ Ibid. 1834, p. 85.

Part II.

When a solution of acetate of potash is employed, hydrog is evolved at the negative pole by the decomposition of wate while the oxygen at the positive pole, acting on acetic aci produces carbonic acid, carbonic oxide, and carburett hydrogen.*

From the facts stated in this chapter, it appears that the electricity evolved in the Voltaic battery is the cons quence of the solution of the zinc in the acid or saline sol tion used; that for every atom of water (or other electrolyt decomposed, an atom of zinc must be dissolved in each cel hence it is evident, that a battery, however large, evolves more electricity than what is given out by a single pair plates of the same size as those in the battery, the effect the great number of pairs is merely to increase the intensit Now, as a certain degree of intensity is requisite to decom pose an electrolyte, it happens that the low intensity of th electricity from a single pair, or a very small number of pair may circulate without decomposing the electrolyte. But M Faraday found that a single pair of plates gives a spark ju when contact is restored.†

Electricity heats and fuses wire.

When electricity passes through perfectly conducting bodie it produces no sensible effect upon them. But if we dimini the size of the conducting body, and augment the quantity electricity so much, that difficulty begins to be encountere when so large a quantity of electricity is obliged to pa through so small a body, effects begin at last to be perceptibl Suppose the conducting body to be a metallic wire. Th wire becomes hot. If the quantity of electricity be increased or the diameter of the wire be diminished, it becomes m hot. If the electricity be still farther increased, the wir melts and is dissipated in smoke. The wire is heated equall over its whole length, unless its extremity is attached to better conductor, in which case it is colder, or to a wors conductor, in which case it is hotter than the other parts.

If a given current of electricity heats a wire a certain num ber of degrees, twice the current will produce doubled th

* Faraday, Phil. Trans. 1834, p. 85. † Phil. Trans. 1834, p. 44

effect, thrice the current triple the effect, and so on. For example, let us suppose a current which moves the Galvanometer 10°, to raise the temperature of a wire 10°, another current which causes a deviation in the Galvanometer amounting to 20°, will heat the wire 20°, and so on. From this it is easy to see that it is the quantity of electricity *passing* through a wire, and not the quantity stopped that raises its temperature.

It has been ascertained by experiments which appear satisfactory, that the ratio of the current to the temperature is as 2 : 3; that is to say, if the current be doubled, or the section of the wire reduced to one half, the augmentation of temperature is tripled.

When negative electricity passes from a better conductor to a worse, the temperature rises; when positive electricity passes from a better conductor to a worse, the temperature sinks. Thus, when negative electricity passed from zinc to iron, the temperature rose 30°; when positive electricity passed from zinc to iron the temperature fell 13°. It would appear from this, that some currents instead of evolving heat, occasion cold.*

The very same effects are produced upon wires, by Voltaic electricity, and common electricity; and in producing these effects, the power of the Voltaic battery depends, not upon the number of pairs of plates, but upon the extent of surface. So that a single pair, supposing the surface equally great, will be capable of igniting as much wire, as any number of pairs of plates, constituting altogether no greater extent of surface than the single pair. The reason is evident. The effect upon the wire must depend upon the absolute quantity of electricity that passes through it in a given time. The intensity of the electricity accumulated in a single pair of plates, of however large dimensions, is so small that it will not give sensible shocks, and will scarcely pass through liquids; yet its absolute quantity is as great as that which is accumulated in 200 pairs of plates: hence, when it passes through a wire, it will exhibit as great an effect upon that

* Peltier, Ann. de Chim. et de Phys. lvi. 371.

Part II.

wire, as would be produced by a battery composed of 200 pairs of plates.

Calorimotor.

Dr Hare, of Philadelphia, has constructed an apparatus which he calls *calorimotor*. It consists of a number of plates of zinc, and as many of copper, about 18 inches square each. The copper plates are all placed in contact with each other, by means of ribbons of metal soldered to them. The same is the case with the zinc plates. These plates are placed alternately at a small distance from each other in a frame. When plunged into a conducting liquid, they constitute, in fact, only a single pair of zinc and copper plates of enormous dimensions. When the zinc is brought into contact with the copper, by means of a platinum wire, of very considerable size, it produces the ignition of that wire, for a length of about two inches, and keeps it ignited as long as the apparatus retains its activity. Yet this apparatus, though so powerful in igniting wire, gives no shocks, and produces no sensible chemical effect whatever. These facts have led Dr Hare to suppose, that the Voltaic battery produces two distinct and unconnected effects; namely, the *evolution of electricity*, which, in his opinion, occasions the shocks and the chemical decompositions, and which depends on the number of pairs of plates, and the *evolution of heat*, which occasions the ignition of metallic wires, and is proportional to the extent of surface in action. But the considerations stated above, afford a satisfactory explanation of the effects of this apparatus, without any necessity to have recourse to the notion, that a current of heat passes through the Voltaic battery, as well as a current of electricity. The chemical effects produced by electricity must, of necessity, depend upon its intensity; but the property which it has of igniting metals must depend upon its absolute quantity. In the calorimotor the intensity is a minimum; being only that produced by a single pair of plates; but the absolute quantity may be as great as we please, as it depends upon the extent of the surface of the two plates, opposite to each other.

We shall terminate this chapter with an account of several pieces of apparatus indispensable to Voltaic investigations:—

Voltameter.

1. *Voltameter.*—This is a glass tube, shut at one end and open at the other. Through the shut end passes a platinum wire, which terminates in a small platinum plate. This tube is filled with a solution of dilute sulphuric acid; and another plate of platinum attached to a wire being introduced into the bottom of the tube, the two wires are connected with the battery so as to bring the tube into the circuit. The water is decomposed, and the two gases collected, and the quantity of gas evolved in a given time measures the powers of the battery at the time.

Electro-multiplier.

2. *Electro-multiplier.*—This very important instrument was first suggested by Professor Schweigger soon after the discovery of the connexion of electricity and magnetism. It consisted originally of a magnetic needle suspended on a pivot, round which, but at a little distance from the needle so as not to touch it, was wrapt a number of coils of copper wire, covered with silk thread, so as to prevent contact. When the Voltaic electricity was made to pass through this wire, the needle deviated to the right or the left according to the direction of the current, and by this deviation marked the direction; while the amount of deviation indicated the intensity, or at least the quantity of electricity which passed. This simple instrument was gradually improved upon. Its sensibility depends upon the number of revolutions which the copper wire makes round the needle. When these coils are numerous, it becomes sensibly affected by a very feeble electric current: hence the reason why it is called a *multiplier*. The sensibility also increases with the delicacy of the suspension of the needle: hence it is occasionally suspended by a single fibre of raw silk. If the magnetic action of the earth upon the needle be neutralized, by suspending two needles in contact with each other, the north pole of the one being turned in the same direction as the south pole of the other, the sensibility is considerably increased. The number of coils, and the size of the copper wire, vary according to the purpose to which it is applied. Sometimes the wire is very long, and of a very small diameter; sometimes it is short, and of a diameter of 0·039 inch, or at least of half that size. Becquerel

Part II. distinguishes these two varieties by calling the first a multiplier with a *long wire*, and the other a multiplier with a *short wire*. Nobili was the contriver of the double needle to remove the influence of the magnetism of the earth. We shall here give the description and figures of two multipliers from M. Becquerel.*

Fig. 1.

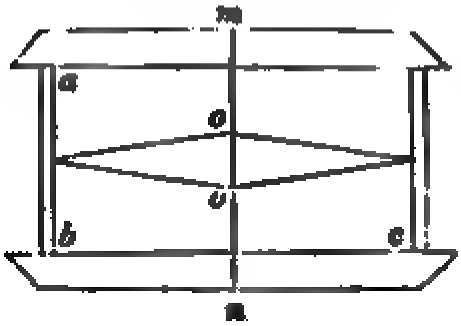


Fig. 5.

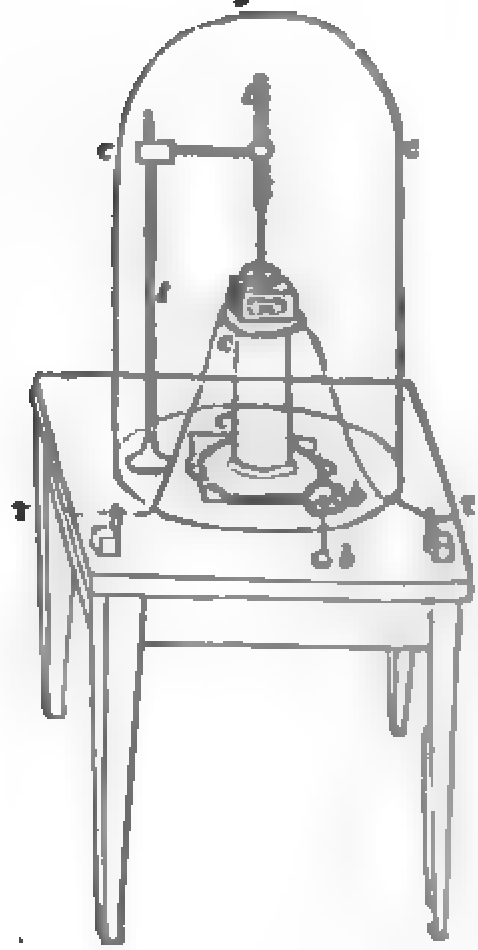
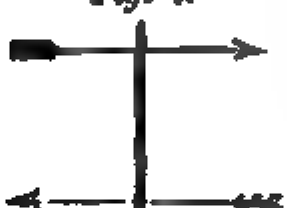


Fig. 2.

Fig. 3.

Fig. 4.



Dimensions of the wooden frame round which the copper wire is coiled.

Fig. 1. Plane. Breadth, $a\ b = 1{\cdot}063$ inches; length, $b\ c = 1{\cdot}575$ inches; opening, $o\ o = 0{\cdot}2$ inch.

Fig. 2. Section in the direction of $m\ n = 0{\cdot}393$ inch.

Fig. 3. Elevation, $l\ l = 0{\cdot}157$ inch.

* Traité de l'Electricité, ii. 16.

The magnetic needles, fig. 4, are two common sewing needles, 1·42 inch in length, and magnetized to saturation. They are placed parallel to one another at the distance of 0·6 inch from each of the extremities of two copper wires, twisted the one upon the other, that they may be deranged when necessary. The needles are suspended by a fibre of raw silk 3·94 inches long.

Whatever precautions be taken, an appreciable directing force still remains; but it does not hinder the apparatus from having a great deal of sensibility. When this directing force is too great, it may be diminished by a contrivance suggested by M. Nobili:—That one of the four poles of the two needles which has the greatest quantity of magnetism is ascertained, a part of its magnetism is removed by rubbing it gently with the opposite pole of a weak magnet; and this is continued till the system leaves the magnetic meridian, and approaches to a direction perpendicular to it; the system is then made to oscillate, and we judge from the number of oscillations in a given time if the magnetism of the earth be sufficiently diminished. It may be laid down as a general rule, that just the directing force should be left, which is sufficient to cause the system to remain in a fixed position, in order that it may leave that position by the action of a very feeble electric current. The whole sensibility of the apparatus depends upon this. The frame should be placed upon a moveable support that we may be able to give the needle every possible position. We gain this object by placing it on a brass cylinder *c c*, (fig. 5,) which turns on its axis by means of a wheel and pinion represented in the figure. By turning the button *b b*, we cause the galvanometer to assume all the positions wanted. The apparatus is placed upon a small table. The raw silk fibre is suspended from a rod *t t*, which can be adjusted at pleasure. The instrument is covered by a glass to screen it from the action of the air.

Multiplier with short wire, or *thermo-multiplier*. Dimensions of frame:—Fig. 1. Plane; breath $a\ b = 1·97$ inch; length, $b\ c = 1·97$ inch; opening, $o\ o = 0·315$ inch. Fig. 2. Section according to $m\ n = 0·63$ inch. Fig. 3. Elevation,

Part. II. $ll = 0{\cdot}1575$ inch. Diameter of copper wire covered with silk, 0·26 inch. It makes thirty turns round the galvanometer. The magnetic needles are 1·8 inch long, and are placed as in the preceding Galvanometer.

Instead of two needles, four may be employed, which are placed in the same way parallel to each other. The needles in the middle have their poles of the same name pointing the same way. The two extreme needles, also, have their poles of the same name directed towards the same point, but turned the opposite way from the two central needles.

The frame ought to have an opening at *o o* (fig. 2.) to allow the four needles to pass. The two middle needles are placed in the interior, and the two extreme needles, the one within and the other without. Becquerel has employed this contrivance advantageously, but the construction requires more care than when two needles only are used.

All that is observed when these multipliers are used, is the number of degrees which the needle deviates from its original position; but the relation between these deviations and the intensity of the electrical currents differs according to the construction of the multiplier. The law of the intensity is very complicated, and could not be understood without entering into mathematical details not suited to this work. The reader will find an account of various methods of deducing the intensity from the deviations, in Becquerel's *Traité de l'Electricité*, (ii. 20,) to which the reader is referred.

CHAPTER XVII.

OF ELECTRO-MAGNETISM.

In this chapter I shall give as concise an account as possible of the recently discovered facts which have shown the dependency of magnetism on electricity, because they are necessary

to complete the sketch which I proposed to give of the present state of the science of electricity. Ch. XVII.

SECTION I.—SKETCH OF THE PHENOMENA OF MAGNETISM.

Magnet what.

The term *magnet* or *loadstone* is usually applied to an ore of iron composed of two atoms of peroxide, and one atom of protoxide of iron. This ore has the colour of iron, the metallic lustre, a specific gravity of about 5, and it crystallizes in octahedrons. It is usually called *magnetic iron ore* by mineralogists. This ore has usually the property of attracting iron filings to itself. Pieces of it that have lain in the earth in a particular direction, are found endowed with *two* or *more poles*, which alternately attract and repel the poles of a magnetic needle. It is to such pieces that the name of *magnet* is applied.*

Artificial magnet.

If a loadstone, possessed of two poles, be drawn along a bar of steel two or three times, always in the same direction, the steel bar will become a magnet. Or if we place a bar of steel in a direction parallel to the axis of the earth, or rather parallel to the dip of the needle, and strike it smartly while in that position, for some time; it will acquire magnetic properties. These properties may be communicated to other bars, and the magnetic virtue in each may be increased by methods described by all writers on magnetism, and therefore generally known.

Poles.

1. If a steel bar, or a steel wire thus converted into a magnet, be suspended by the middle by a slender string, or supported on a pivot, on which it can turn easily, one end of it will always point towards the *north*, and the other end towards

* It was called by the Greeks μαγνης, μαγνητης, and μαγνητις, according to some, from the name of a shepherd, who first discovered it on mount Ida. Theophrastus does not notice it in his treatise περι λιθων; but it is mentioned by Aristotle, and probably other Greek writers. Pliny treats of it under the name of *magnes*, and gives an account of its property of attracting iron. Its polarity was unknown to the ancients, nor is it known by whom it was discovered. It was certainly known in the 12th, or beginning of the 13th century. The mariner's compass was introduced by Flavio Gioia, or Giri, of Melphi, in the year 1302.

Part II.

the *south*. If we make it deviate from this position, it will oscillate for some time, and at last settle in the same position as at first. That extremity of the bar or wire which points to the *north*, is called the *north pole* of the magnet, and the other extremity which points to the south, is called the *south pole*.

Of the same name repel, of different attract.

2. If we present the north pole of one magnet to the north pole of another magnet, both capable of moving freely upon their axes, they *repel* each other. In like manner the two south poles repel each other. But the north pole of one magnet *attracts* the south pole of another, and *vice versa*. Coulomb demonstrated that these attractions and repulsions vary inversely as the square of the distance.*

3. If we determine the direction of a magnetic needle, properly suspended, and at liberty to move, we shall find, in general, that it does not point due north, but either to the west or east of north. The exact point of the compass to which it points, varies in different parts of the earth. For instance, if we sail from the Straits of Gibraltar to the West Indies, in proportion as we recede from Europe and approach America, the compass will point nearer and nearer due north; and when we come to a certain part of the Gulf of Mexico, it will point exactly north. But if we sail from Great Britain to the southern coast of Greenland, we shall find the needle deviate farther and farther from the north as we approach Greenland, where the deviation will not be less than 45° or 50° west from north.

Declination of the needle.

Even if we examine a compass stationed in the same place it will not always continue to point exactly in the same direction. Suppose, when we first begin to make our observations, it were to point due north, after a certain number of years, if we again observe the direction, we shall find it to point several degrees *west* or *east* from north. This change in the direction of the compass in the same place is called the *variation* or *declination*. According to Dr Halley, it was discovered by Gellibrand in 1645; and according to Bond, it was first noticed by Mr John Mair. The following little table will show

* Mem. Paris, 1785, p. 587.

variation of the compass at London from the time that it Ch. XVII.
first observed, or at least recorded :—

Years.		Variations.					Observers.
1580	.	11°	15′	0″	East	.	Barrows.
1622	.	6	0	0	——	.	Gunter.
1634	.	4	5	0	——	.	Gellibrand.
1657	.	0	0	0	.	.	Bond.
1672	.	2	30	0	West	.	Halley.
1682	.	4	30	0	——	.	——
1692	.	6	0	0	——	.	Halley.
1722	.	14	20	0	——	.	Graham.
1747	.	17	40	0	——	.	Graham.
1774	.	21	16	0	——	.	Cavandish.
1786	.	23	17	0	——	.	Gilpin.
1790	.	23	39	0	——	.	Gilpin.
1796	.	24	0	0	——	.	Gilpin.
1800	.	24	3·6	0	——	.	Gilpin.
1809	.	24	11	0	——	.	Gilpin.
1814	.	24	21	10	——	.	Lee.
1815	.	24	17	5	——	.	Lee.
1816	.	24	17	54	——	.	Lee.
1817	.	24	17	0	——	.	Lee.
1818	.	24	15	43	——	.	Lee.
1819	.	24	14	47	——	.	Lee.
1820	.	24	11	44	——	.	Lee.
1821	.	24	11	18	——	.	Lee.
1822	.	24	9	55	——	.	Lee.
1823	.	24	9	48	——	.	Lee.

Ve see from this table that it reached its greatest western iation in 1814, or 157 years after the needle was observed Bond to point due north. Since 1814, it has been moving ly eastward; and if it take as many years to return as it to proceed westward, it will reach the point of no deviation e year 1971. Should it go as far to the eastward as it did tward, and take as long a time, it will reach the eastern- t declination in the year 2128. The total arc of declina- will be 48° 35′ 48″, and the period occupied in passing the whole of it will be 314 years. This would be an ration in the variation amounting at an average to 9′ 17″

Part II. annually. But it is much smaller than this towards its western and eastern limits, while it is much more rapid when it deviates but little from due north. Thus during the nine years that elapsed between 1814 and 1823, the progress eastwards is only 11′ 22″ or only 1′ 1″·6 annually. While from 1657 to 1672, the declination west amounted to 2° 30′, or 10′ annually. Between 1672 and 1682, the increase amounted to 2°, or 12′ annually. It was the same nearly between 1682 and 1692. Between 1692 and 1722, the average annual increase of declination was 16′ 40″. This was the maximum. After the year 1722, the rate diminished very rapidly. It seems to have reached half way, or about 12° of western declination about the year 1714, or in 57 years. To complete the other half 100 years were required. These circumstances render it impracticable to calculate the length of the period of the variation from any data in our possession.*

Earth has four poles. The first person who attempted to account for the declination of the needle, was Dr Halley. He considered the globe of the earth as constituting one great magnet, having four poles, two to the north and two to the south, at considerable and unequal distances from the poles of the earth. Two of these poles (one to the north, the other to the south.) are fixed. The other two are moveable. And to account for the motion of these last, he considers them as the poles of an internal globe concentric with that of the earth, and not moving with the same velocity round its axis; so that its poles gradually fall behind those of the external globe.† This subject has been investigated with much ingenuity by Professor Hansteen, of Christiania, who has had the advantage of another century, and a prodigious number of new observations. He has adopted the Halleyan hypothesis, that the earth is a magnet with four poles, two north and two south. One of the north poles and one of the south are much weaker than the other two. The action of these poles on each other occasions a slow change in their position, which is followed of necessity by a change in the declination of the needle. Hansteen calculates that these

* We have inserted Hansteen's chart of the dip opposite to page 48[illegible].

† Halley; Miscelanea Curcosa, vol. i. pp. 27. 43.

poles revolve round the pole of the earth, the one in 1740 and the other in 860 years. Ch. XVII.

The strongest north pole has been determined by Captain Ross to be situated in north latitude 70° 5′ 17″, and west longitude 96° 46′ 45″ near the southern extremity of Prince Regent's inlet, a gulf running south from Barrow's Straits.*

The weakest north pole Hansteen considers as situated in north latitude 85° 12′, and east longitude 140° 6′. This is in the Arctic Sea, to the east of Nova Zembla, and not quite 5 degrees from the terrestrial pole. But Hansteen not considering the position of this pole as determined with sufficient accuracy, made a journey to Siberia in order to settle it from a sufficient number of actual observations on the declination and dip of the needle, in different stations on the northern shores of Europe and Asia.

The strongest south pole, according to Hansteen, is situated in south latitude 68° 52′, and east longitude 132° 35′, not far from the meridian of Van Dieman's Land, though greatly to the south.

The weakest south pole is in south latitude 78° 16′, and west longitude 135° 59′. It lies in the great South Sea, almost as far to the west of Cape Horn as that promontory is west from London, and greatly within the polar circle, being less than 12 degrees from the south pole. These positions of the magnetic poles of the earth differ a good deal from those assigned by Halley, and are certainly much nearer the truth. Hansteen was enabled to attain this approximation to accuracy by the vast number of magnetical observations that have been made in all parts of the world during the latter part of the last century, and the portion of the present which has elapsed, chiefly by Captain Cook and the other commanders of the numerous voyages of discovery, which have been sent out at different times by the British government.

Daily variation.

4. In the year 1722, Mr Graham discovered that the magnetic needle has a daily variation. Mr Canton made a few observations on this daily variation, and gave the following as

* Phil. Trans. 1834, p. 47.

Part II. the mean monthly daily variations at London for the year 1759 :—

January	7′	8	July	13′	14″
February	8	58	August	12	19
March	11	17	September	11	43
April	12	26	October	10	36
May	13	0	November	8	9
June	13	21	December	6	58

But the most complete set of observations on the daily variation of the needle was made by Colonel Beaufoy. He observed three times a-day, about half-past eight in the morning, at noon, and about seven in the evening, and he continued his observations for several years without interruption.* The declination was always least in the morning, and greatest at noon. The mean of observations kept by him for 2 years and 6 months, give the declination as follows :—

Morning . .	24°	14′	39″
Noon . .	24	21	54
Evening . .	24	16	4·5

From Canton's observations, it would appear that the declination increases with the heat of the weather; but Colonel Beaufoy did not find this rule to hold in his observations.

More lately the subject has been taken up by Mr Barlow and Mr Christie, who have published important observations on the diurnal variation of the needle in the *Philosophical Transactions*. They observe that the needle begins to decline to the east early in the morning, and obtains its greatest deviation eastwards about seven in the morning. The needle then begins to move westwards to about half-past ten o'clock, when the diurnal variations are zero. The needle now begins to decline westwards, and continues to do so till about half-past one, or two o'clock, when it has reached its maximum. It then begins to diminish, the needle again moving eastward.

* These valuable observations will be found in the *Annals of Philosophy* (1st series), beginning at vol. ii., and continuing till the end of vol. vi. They were again resumed in vol. ix. p. 390, and continued till vol. xvi. including a series of three years and nine months. In the *Annals of Philosophy* (new series), i. 94, will be found a summary of all his observations, drawn up by Colonel Beaufoy himself.

At about 5 o'clock the deviation again vanishes, and the needle points again to the magnetic north. It continues to move east till late in the evening.*

Dip or inclination.

5. If a steel wire, supported horizontally upon its centre of gravity, be converted into a magnet, it will no longer remain horizontal. The north pole dips down towards the earth, while the south pole is elevated. This is called the *dip* or *inclination* of the magnetic needle. It was first observed by Robert Norman, in 1576. He constructed an instrument to measure the angle which such a needle made with a supposed horizontal needle, and he states it for London at 71° 50′. If we carry such a needle south towards the equator, we observe

* The following table exhibits the mean monthly variation of the compass, from April 1817, to March 1819, as determined by Colonel Beaufoy :—

From April, 1817, to March, 1819.		Years 1817 and 1818, westerly variation.	Years 1818 and 1819, westerly variation.	Differ. in morning, noon, and evening.	Difference in 1817 and 1818.	Difference in 1818 and 1819.	Mean of the two.
April	Morning	24° 31′ 52″	24° 34′ 06″	*n* — *m*	12′ 51″	10′ 44″	11′ 48″
	Noon	— 44 43	— 44 50	*n* — *e*	8 45	8 14	8 30
	Evening	— 35 58	— 36 36	*e* — *m*	4 6	2 30	3 18
May	Morning	— 32 20	— 36 18	*n* — *m*	10 15	9 31	9 53
	Noon	— 42 35	— 45 49	*n* — *e*	7 50	7 14	7 32
	Evening	— 34 45	— 38 35	*e* — *m*	2 25	2 17	2 21
June	Morning	— 31 09	— 33 47	*n* — *m*	11 5	11 24	11 15
	Noon	— 42 14	— 45 11	*n* — *e*	8 9	7 31	7 50
	Evening	— 34 05	— 37 40	*e* — *m*	2 56	3 53	3 25
July	Morning	— 31 14	— 34 24	*n* — *m*	10 52	10 35	10 43
	Noon	— 42 06	— 44 59	*n* — *e*	6 23	6 45	6 34
	Evening	— 35 43	— 38 14	*e* — *m*	4 29	3 50	4 9
Aug.	Morning	— 31 16	— 34 40	*n* — *m*	11 35	11 18	11 26
	Noon	— 42 45	— 45 58	*n* — *e*	9 6	8 8	8 34
	Evening	— 33 45	— 37 50	*e* — *m*	2 29	3 10	2 52
Sept.	Morning	— 33 02	— 34 24	*n* — *m*	8 34	10 53	9 44
	Noon	— 41 36	— 45 22	*n* — *e*	6 58	7 54	7 26
	Evening	— 34 38	— 37 28	*e* — *m*	1 36	2 59	2 18
Oct.	Morning	— 31 06	— 35 36	*n* — *m*	9 40	7 52	8 46
	Noon	— 40 46	— 45 28				
Nov.	Morning	— 31 49	— 33 24	*n* — *m*	6 6	8 17	7 10
	Noon	— 37 55	— 41 41				
Dec.	Morning	— 34 03	— 37 04	*n* — *m*	3 59	4 16	4 07
	Noon	— 38 02	— 41 20				
Jan.	Morning	— 34 02	— 35 42	*n* — *m*	5 55	4 12	5 03
	Noon	— 39 57	— 39 54				
Feb.	Morning	— 34 22	— 34 17	*n* — *m*	6 29	5 38	6 03
	Noon	— 40 51	— 39 55				
March	Morning	— 33 18	— 33 18	*n* — *m*	8 19	8 24	8 22
	Noon	— 41 37	— 41 42	*n* — *e*	7 50	6 25	7 07
	Evening	— 33 47	— 35 17	*e* — *m*	0 29	1 59	1 15

Mr Barlow has given a very ingenious and plausible explanation of the daily variation, by supposing the sun to possess a certain magnetic action on the needle. See his *Essay on Magnetic Attractions*, p. 96.

Part II. that the dip diminishes in proportion with the diminution of latitude; till at last, at a certain point of the earth's surface, constituting the magnetic equator, the needle has no dip at all, but stands perfectly horizontal. When we pass this equator. the dip begins again to make its appearance; but it is the south pole which is now sunk towards the earth, while the north pole is elevated, and the dip of the south pole increases as the latitude increases.

To understand the reason of this dip, we have only to recollect that the earth is a magnet, and that its poles are situated below its surface. The directive property of the needle, is owing to the attraction of these poles. When the needle is on the north side of the magnetic equator, the earth's north poles will have the greatest effect, and the needle will point directly to that north pole, which is so situated as to have the greatest intensity relative to the needle. If the needle was directly over that pole, its north pole pointing to it would cause the needle to assume a position perpendicular to the horizon. The farther south it is, the greater will be the action of the earth's south poles. This will occasion a diminution of the dip. At the magnetic equator both the north and south poles will act with equal energy on the needle. The consequence must be that it will assume a direction perfectly horizontal. To the south of the magnetic equator, the south poles of the earth will act more powerfully upon the needle than the north poles: hence the south pole of the needle will begin to dip, and this dip will increase as we advance southwards.

As the magnetic poles do not coincide with the terrestrial poles, it is obvious that the magnetic equator cannot coincide with the terrestrial equator. It was long believed, that the magnetic equator was a great circle, the plane of which was inclined to that of the terrestrial equator at an angle of about 12°; that it cut the equator in two points or nodes, one of which was situated in about west longitude, 115° 24′ 44″, considerably to the west of the Galipagos islands, situated near Columbia in South America, and the opposite node in west longitude 295° 24′ 44″, situated in the Indian Ocean; but the numerous magnetic observations made by Captain Cook

in his various voyages enabled Biot to show that this opinion was ill founded.* It corresponded indeed with the magnetic observations made in the Atlantic and Indian oceans, and in Europe and the coasts of Africa and America; but was quite irreconcileable with those in the Pacific Ocean between west longitude 115° and 270°, which comprehends little less than a hemisphere. He showed that Cook and Bayley crossed the magnetic meridian in west longitude 158° 41′ 53″, and in 3° 13′ 40″ south latitude. Biot concluded from this, that there were three or probably four nodes or crossings of the terrestrial equator by the magnetic equator.

M. Morlet, by a peculiar method of interpolation, determined the position of these nodes, and likewise the true form of the magnetic equator. M. Hansteen also determined the position of the magnetic equator, founding his calculations, as M. Morlet had done, upon observations made in 1780. M. Duperrey afterwards fixed the position of the magnetic equator, from the numerous observations made by him in his voyages of discovery in the years 1822, 1823, 1824, and 1825.†

Though the data employed by Morlet and Hansteen were the same, some slight differences appear in their determinations. The magnetic equator is placed by both wholly to the south of the equinoctial line, in the Atlantic Ocean, between Africa and America. In west longitude 24° 52′, it is 13° or 14° to the south; in the chart of Hansteen, it crosses the equator about 22° 8′ of east longitude. M. Morlet places the node about 4° farther west.

Setting out from this node and advancing into the Indian Ocean, the line of no inclination rises rapidly to the north of the equator; it leaves Africa a little to the south of Cape Guardafan, and reaches its maximum of northing (about 12°) in 62° 8′ of east longitude. Between this meridian and 174° 8′ of east longitude, the magnetic equator is always in the northern hemisphere. It cuts the peninsula of Hindostan a little to the north of Cape Comorin; traverses the Bay of Bengal, inclining slightly to the equator, from which its average

* Traité de Physique, iii. 128. † Poggendorff's Annalen, xxi. 151.

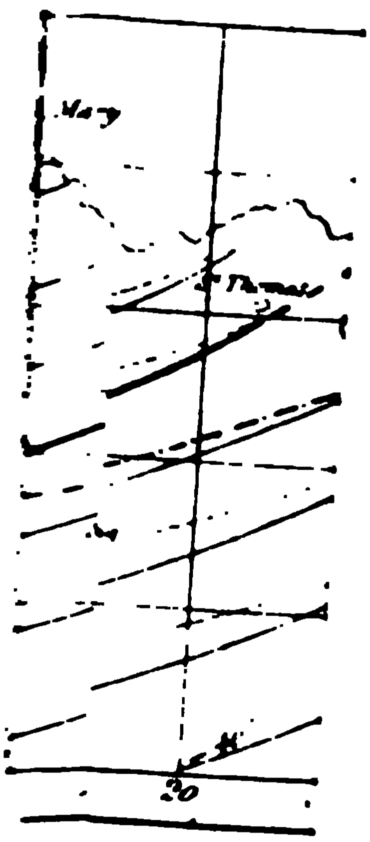
Part II. distance is about 8° at the entrance of the Gulf of Siam, advances a very little to the north, and is almost parallel to the equator at the northern extremity of Borneo and for a considerable way east. It passes through the Island of Paraguay, through the strait which separates the most southerly of the Philippines from the isle of Mindanao, and at the meridian of Waigiou is 9° to the north of the equator. After passing through the Caroline isles, it inclines rapidly to the south, and cuts the terrestrial equator according to Morlet in 174°, and according to Hansteen in 187° of east longitude. There is considerable uncertainty about the position of another node situated also in the Pacific Ocean. It ought to be not far from 120° of west longitude. According to Morlet, the magnetic equator, after touching the terrestrial equator, inclines again to the south; but Hansteen supposes that it crosses the terrestrial equator, and continues in the northern hemisphere during an extent of about 15° of longitude, and then cuts the equator again at about 23° to the west of the continent of America. In Hansteen's chart, the magnetic equator in that part of its course does not deviate more than 1½°, so that it nowhere differs more than 2° from the position laid down by Morlet.*

It is obvious from Morlet's deductions, that the magnetic equator has a movement of translation from east to west. The two nodes of Hansteen and the tangent of Morlet in the Pacific are situated between west longitude 108° and 126° calculated for 1780; but M. Freycinet found that in 1819, it was in west longitude 132°. According to Captain Sabine, the point of intersection of the two equators, which in 1780 was in the interior of Africa, has now advanced from east to west into the Atlantic Ocean.†

Morlet stated, it is probable that the position of the magnetic equator regulates from one pole to the other the direction of the annual variations of the magnetic needle. If we call the distance of any point from the magnetic equator its mag-

* We have inserted here, to make the position of the magnetic [illegible] intelligible, Hansteen's chart both of the dip and declination of the needle.

† Morlet's chart will be found in the Mémoires des Savants Etrangers, t. iii. p. 182; Hansteen's and Duperrey's in Poggendorff's Annalen, t. xi [illegible]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Thomas
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netic latitude, measured on the magnetic meridian, considered as a grand circle, he found that the inclination of the needle diminished with the diminution of that latitude, and increased with its augmentation. This has been confirmed by subsequent observations. Ch. XVII.

The dip of the needle, or the *inclination*, in the same place, undergoes a slow change as well as the declination, and doubtless depending upon the same cause, the gradual alteration in the position of the earth's magnetic poles. The following table exhibits the amount of the dip in London, during an interval of 254 years, according to the best observations which have been made :—

Years.	Dip.	Observers.
1576	71° 30′	Norman*
1600	72 00	Gilbert
1676	73 47	Bond*
1720	75 10	Whiston*
1723	75 00	Graham*
1772	72 19	Nairne,†
1776	72 30	Cavendish‡
1805	70 21	Gilpin§
1821	70 3	Sabine‖
1830	69 37·5	

From this table it appears, that the dip reached its maximum in London, about the year 1720, and that it has been diminishing ever since. But the difficulty of constructing an accurate dipping needle is so great, that all the observations hitherto made, can be considered only as approximations.

The following table exhibits the amount of the dip at Paris, in N. latitude 48° 50′ 14″, and East longitude from Greenwich 9′ 21″·6¶ during a series of 147 years :—

Years.	Dip.	Observers.
1671	75° 0′	Picard
1754	72 15	La Caille

* As given by Cavendish, in his paper on the Meteorological Instruments of the Royal Society. Phil. Trans. 1776, p. 375. Some of Mr Graham's observations only gave a dip of 73° 30′.

† Phil. Trans. 1772, p. 476. ‡ Ibid. 1776, p. 375.

§ Ibid. 1806. Meteorological Table. ‖ Ibid. 1822, p. 1.

¶ See Phil. Trans. 1826, p. 77.

Part II.

Years.	Dip.	Observers.
1776	72° 25′	Le Monnier
1780	71 48	Cassini
1798	69 51	Humboldt
1810	68 50	*
1818	68 35	*

If we except the observations of Le Monnier, the dip l been constantly diminishing at Paris since the year 167 when it probably exceeded the dip in London.

Morlet has given us the variation of the dip in vari places. It will be worth while to insert here a few of l tables.†

COPENHAGEN.

Years.	Dip.	Observers.
1773	71° 45′	Lous
1786	71 30	Löwenörn
1791	71 20	Bugge
1813	71 26	Wieugel

BERLIN.

1805	69° 53′	Humboldt
1826	68 40	do.
1829	68 30	do.

TENERIFFE.

1792	62° 25′	Rossel
1799	62 25	Humboldt
1801	62 26	Bernier
1822	59 50	Sabine
1822	57 6	Duperrey

ISLE OF ASCENSION.

1752	11° 10′	La Caille
1774	8 57	Wales
1822	5 10	Sabine
1825	1 58	Duperrey

* These two copied from Despretz Traité de Physique, p. 446.
† Mem. des Savants Etrangers, de l'Institut de France, iii. 176.

ST HELENA.

Years.	Dip.	Observers.
1775	11° 25′	Wales
1825	15 3	Duperrey

CAPE OF GOOD HOPE.

1751	42° 58′	La Caille
1774	45 37	Bayley
1774	45 19	Wales
1776	46 31	Bayley
1776	45 6	Cook
1780	46 45	Bayley
1780	45 27	King
1792	48 30	Vancouver

ISLE OF FRANCE.

1754	52° 17′	La Caille
1801	54 29	Bernier
1824	53 47	Duperrey

OTAHEITE.

1773	29° 43	Bayley
1774	29 47	do.
1777	29 47	do.
1777	29 12	Cook
1823	29 35	Duperrey
1823	30 31	do.

PORT JACKSON.

1802	62° 44′	Bernier
1802	62 52	Flinders
1821	62 36	Brisbane
1824	62 17	Duperrey

AMBOINA.

1792	20° 37′	Rossel
1824	20 32	Duperrey

It would appear from these tables, that the dip is diminishing in the northern hemisphere and augmenting in the southern. This indicates a movement of the magnetic equator from south to north.

Part II.

5. Lines indicating the declination of the needle in different parts of the earth's surface were first published in 1683 by Dr Halley, together with tables of the variation of the compass in various places where observations have been made.* Other charts exhibiting the lines of equal declination of the needle were afterwards given to the public by Yates, Churchman, and especially by Hansteen in the atlas which accompanies his *Untersuchungen über der Magnetismus der Erde*, published in 1819. In 1833, a new declination chart was constructed by Mr Barlow.† It would be impossible to render the position of these lines intelligible without Mr Barlow's chart, to which therefore the reader is referred. On attentively considering these lines of declination on the map it will be acknowledged that notwithstanding their extraordinary curvatures, they exhibit a character which indicates them being dependent on some law, however intricate and mysterious. If they were greatly influenced by parts immediately in their vicinity, we could not find that regularity observable in nearly all of them. Throughout the Atlantic for example, there is a continuity and softness of curvature, and unity of disposition inconsistent with such a supposition.

Lines of equal declination.

In the Indian Ocean, we have a most extraordinary inflection of the curve of no variation, whereby we find in tracing the earth's equator that two-thirds of it, or 240°, have easterly variations, while only one-third, or 120° have westerly variations.

Another marked peculiarity in this ocean is, that for 40° the line of no variation runs nearly parallel to the equator, and then for other 40° down a meridian. It passes from Vansittard Bay, in the north-west part of New Holland, to Sandalwood Island in south latitude 10° and east longitude 120°, passes nearly parallel to the equator to almost east longitude 90°, when it takes a north-west sweep, crosses the equator in east longitude 70°, takes a northerly direction and enters the peninsula of Hindostan about north latitude 20° and about east longitude 69°. It makes its appearance again in the east coast of China, in north latitude 25° and about

* Phil. Trans. xiii. 208. † Ibid. 1833, p. 667.

120° of east longitude. From this point it sweeps rapidly to the north and crosses north latitude 60° at east longitude about 38°.

The western line of no variation crosses south latitude 60°, at about west longitude 20°, and proceeds northerly to Bahia in Brazil, in about 13° of south latitude. From this place, it proceeds nearly in a northern direction to Washington, in the United States, in west longitude about 76°, and north latitude about 38°. It afterwards passes through Hudson's Bay, crossing north latitude 60°, in about west longitude 89°.

The progressive change in the position of the curves of equal variation on the globe, is a most important feature in terrestrial magnetism. Mr Barlow informs us, that the variation itself was first noticed by Petri Peregrini, in 1269, who mentions in a letter to a friend, that after several careful observations, he had found it 5° east in Italy. In 1580, it was 11°¼ east in London; about 1658, it was zero. In 1700, it was about 8° west, at which time it was zero on the coast of America, in the vicinity of New York, and it has remained at the latter place nearly the same ever since. So that about the year 1660, the line of no variation must have crossed the Atlantic, nearly at right angles to the meridian, as it does now the Pacific Ocean.

Very little variation has been observed in Australia during the last fifty years: hence it is probably as fixed there as on the coast of America. In the West Indies, the Bermudas, and a few other places where the variation is small, the change has been inconsiderable. But we know of no place where the variation is large and stationary.

In all places in which sufficient registers of variation have been made, and where the motion or change has been considerable, we may always reduce that motion to a circular rotation of a certain assumed magnetic pole, about the pole of the earth. But if such moveable poles exist, (and the coincidence of calculation and observation leaves little doubt on the subject,) how can we account for those stationary, or nearly stationary points of no variation in different parts of the globe?

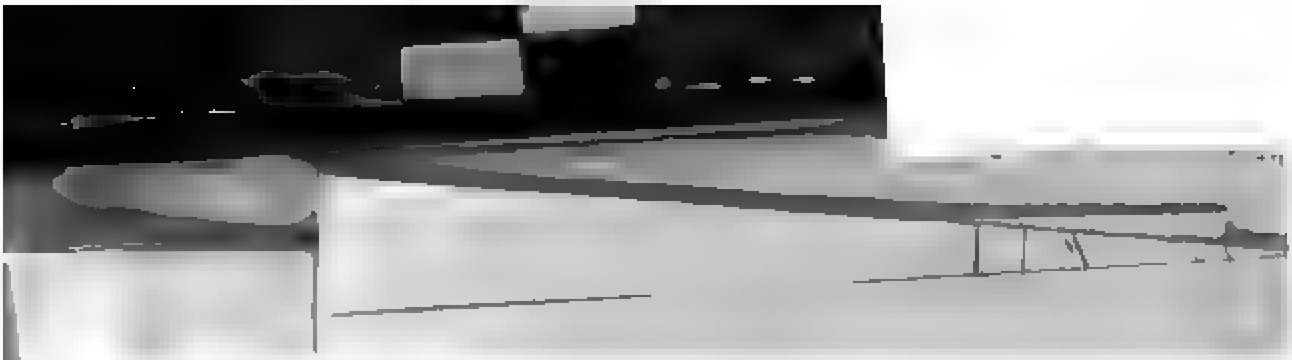
Part II. distance is about 8° at the entrance of the Gulf of Sia vances a very little to the north, and is almost parallel equator at the northern extremity of Borneo and for siderable way east. It passes through the Island of Par through the strait which separates the most southerly Philippines from the isle of Mindanao, and at the meri Waigiou is 9° to the north of the equator. After p through the Caroline isles, it inclines rapidly to the sout cuts the terrestrial equator according to Morlet in 174 according to Hansteen in 187° of east longitude. Th considerable uncertainty about the position of another situated also in the Pacific Ocean. It ought to be not fa 120° of west longitude. According to Morlet, the ma equator, after touching the terrestrial equator, inclines a the south; but Hansteen supposes that it crosses the terr equator, and continues in the northern hemisphere dur extent of about 15° of longitude, and then cuts the e again at about 23° to the west of the continent of An In Hansteen's chart, the magnetic equator in that part course does not deviate more than $1\frac{1}{2}$°, so that it nowhere more than 2° from the position laid down by Morlet.*

It is obvious from Morlet's deductions, that the ma equator has a movement of translation from east to west. two nodes of Hansteen and the tangent of Morlet in the P are situated between west longitude 108° and 126° calc for 1780; but M. Freycinet found that in 1819, it was i longitude 132°. According to Captain Sabine, the p intersection of the two equators, which in 1780 was in t terior of Africa, has now advanced from east to west in Atlantic Ocean.†

Morlet stated, it is probable that the position of the netic equator regulates from one pole to the other the dir of the annual variations of the magnetic needle. If the distance of any point from the magnetic equator its

* We have inserted here, to make the position of the magnetic e intelligible, Hansteen's chart both of the dip and declination of the

† Morlet's chart will be found in the Mémoires des Savants Etr t. iii. p. 182; Hansteen's and Duperrey's in Poggendorff's Annalen.



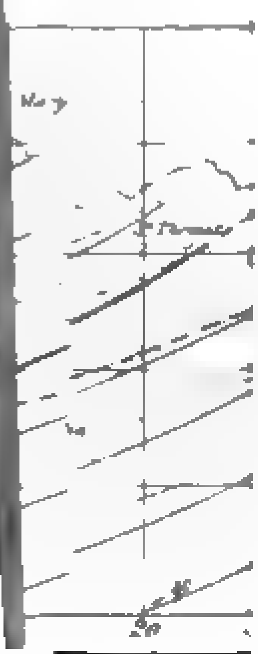
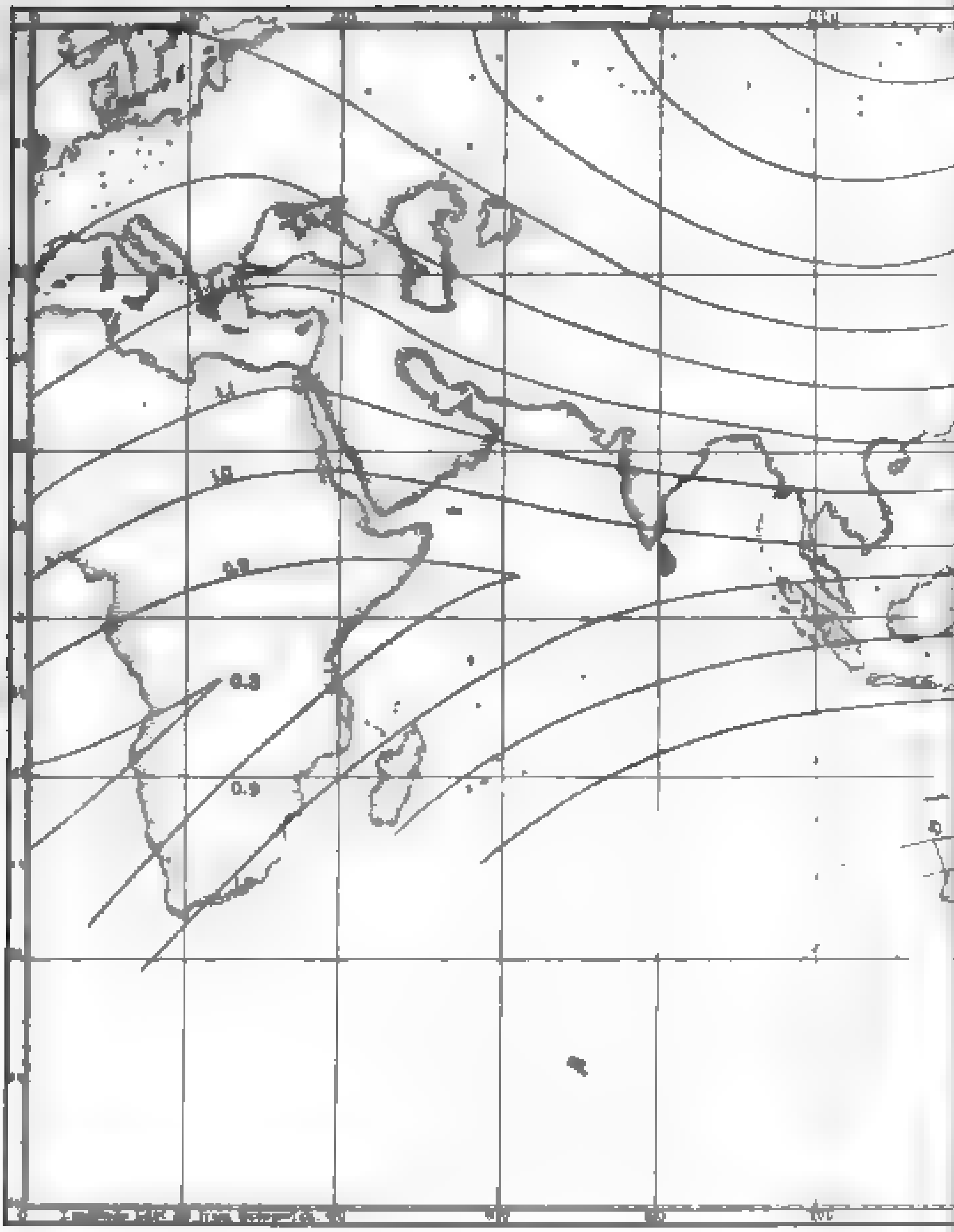


CHART exhibiting



of Africa, Brazil, West Indies, North America, Ch. XVII.
, Spitzbergen, and Norway. Many other similar
is have since been made in various regions of the
ny of which have been consigned to the public in
it volumes of the *Philosophical Transactions.**

ig these experiments, it is necessary that the needles
hould be magnetized to saturation, and the experi-
uld make himself sure that none of the intensity
st, by repeating the vibrations in the place where
;an his experiments. If the needle made the same
vibrations in a given time as it did at first, he may
at no magnetism has been lost during the course
eriments, however long they may have been con-

of attempting an enumeration of all these numerous
s, which would occupy too much room, it will be
give a table of the intensity in different places, as
om these observations by Hansteen, to whom this
science lies under so many obligations.†

Tables of dip and intensities.

Places.	Dip. South.	Intensity.
DE ROSSEL OBSERVER.		
rd, l, } Van Diemen's Land, . .	70° 50′	1·5773
	70 48	1·6133
in Java, ...	25 40	0·9348
. . . .	20 37	0·9532
HUMBOLDT OBSERVER.		
. . . .	[illegible] 59	1·0773
quator in Peru, ...	0 0	1·0000
	North.	
. . . .	3° 11′	1·0191
. . . .	5 24	1·0095

educed copy of Sabine's plate of the lines of equal intensity,
ed.

table may be seen in *Poggendorff's Annalen*, iii. 422. He
rrected table in the same book, tom. vi. p. 321; and a table
orrected, in tom. ix. p. 236. His chart of isodynamic lines
l in *Poggendorff's Annalen*, xxviii. 473.

Part II.

Places.	Dip. North.	Intensity.
Cuença,	8° 43′	1·0286
Quito,	13 22	1·0675
St Antonio,	14 25	1·0871
St Carlos,	20 47	1·0480
Popayan,	20 53	1·1170
Santa Fe de Bogota,	24 16	1·1473
Javita,	24 19	1·0675
Esmeralda,	25 58	1·0577
Corichana,	30 24	1·1575
St Thomas,	35 6	1·1070
Carthagena,	35 15	1·2938
Cumana,	39 47	1·1779
Atlantic, N. lat. 20° 46′, W. long. 41° 26′*,	41 46	1·1779
Atlantic, 11 0, 44 32,	41 57	1·1617
Mexico,	42 10	1·3155
Atlantic, N. lat. 12° 34′, W. long. 33° 14′,*	45 8	1·2300
Atlantic, 14 20, 28 3,	52 55	1·2830
Atlantic, 20 8, 8 34,	56 42	1·2510
Atlantic, 21 36, 5 39,	47 49	1·2617
Atlantic, 25 15, 0 36,	60 18	1·2830
Portici,	60 5	1·2883
Naples,	61 35	1·2745
Rome,	61 57	1·2642
Crater of Vesuvius,	62 0	1·1933
St Cruz, Teneriffe,	62 25	1·2723
Valencia,	63 38	1·2405
Florence,	63 51	1·2782
Atlantic, lat. 32° 16′, long. 2° 52′,*	64 21	1·2938
Barcelona,	64 37	1·3482
Marseilles,	65 10	1·2938
Nimes,	65 23	1·2938
Mailand,	65 40	1·3121
Montpellier,	65 53	1·3482
Airolo, St Gothard,	65 55	1·3480
Turin,	66 3	1·3364
Medina del Campo,	66 9	1·2938
Lans le bourg, on Mount Cenis,	66 9	1·3227
Como,	66 12	1·3104
St Michael,	66 12	1·3488
Lyons,	66 14	1·3334
Hospital of St Gothard,	66 22	1·3138
Hospital of Mount Cenis,	66 42	1·3441
Urseru,	66 53	1·3479
Altorf,	67 30	1·3228
Atlantic, lat. 37° 14′, long. 3° 30′ E.*	67 30	1·3155
Atlantic, 38 52, 3 40,	67 40	1·3155

* From Ferro.

Ch. XVII.

Places.	Dip. North.	Intensity.
Madrid,	67° 41′	1·2938
Tubingen,	68 4	1·3569
Atlantic, N. lat. 38° 52′, E. long. 3° 40′,*	68 11	1·3155
Ferrol,	68 32	1·2617
Paris,	69 12	1·3482
Gottingen,	69 29	1·3485
Berlin,	69 53	1·3703
ŒRSTED, ERICHSEN, HANSTEEN, OBSERVERS.		
Carolath,	68 21	1·3509
Berlin,	68 50	1·3533
Dantzick,	69 44	1·3737
Altona,	69 46	1·3594
London,	69 57	1·3607
Ploen,	70 2	1·3575
Ystad,	70 13	1·3747
Oxford,	70 12	1·3706
Schleswig,	70 19	1·3628
Copenhagen,	70 36	1·3672
Odense,	70 50	1·3782
Helsenburg,	70 52	1·3782
Kolding,	70 53	1·3846
Soroe,	70 57	1·3842
Fredericksburg,	70 59	1·4028
Aarthuus,	71 13	1·3838
Liverpool,	71 14	1·3657
Aalborg,	71 37	1·3780
Odensala,	71 39	1·3666
Frederickshaven,	71 48	1·3842
Gothenhurg,	71 58	1·3826
Altorp,	72 14	1·3891
Korset,	72 24	1·3735
Quistrum,	72 27	1·4047
Skieberg,	72 29	1·3725
Edinburgh,	72 34	1·4005
Ellöen,	72 38	1·3840
Helgeroa,	72 39	1·3980
Soner,	72 41	1·3835
Christiania,	72 36	1·4195
Ryenberg,	72 45	1·4208
Bogstad,	72 34	1·4378
Bogstadberg,	73 13	1·4195
Näsodden,	73 2	1·4517
Barum,	72 44	1·3902
Bolkesjöe,	73 15	1·4058

* From Ferro.

Part II.

Places.	Dip North.	Intensity.
Ingolfsland,	73° 19′	1·4159
Nörsteböe,	73 33	1·4136
Drammen,	73 37	1·3771
Maursäter,	73 44	1·4656
Ullensvang,	73 44	1·4260
Gran,	73 45	1·4221
Kongsberg,	73 47	1·4144
Tomlevold,	73 50	1·4246
Bekkervig,	73 58	1·4114
Vang,	73 59	1·4308
Bergen,	74 3	1·4220
Moe,	74 3	1·4234
Moristuen,	74 4	1·4058
Leiersdal,	74 6	1·4190
Slidre,	74 34	1·4543
Findaas,	74 48	1·4503
CAPTAIN SABINE OBSERVER.		
Brassa, Shetland,	74 21	1·4471
Davis' Straits, N. lat. 68° 22′, W. long. 36° 10′,	83 8½	1·6365
Hosen Island, lat. 70° 26′, W. long. 37° 12′,	82 49	1·6496
Baffin's Bay, lat. 75° 5′, W. long. 42° 43′,	84 25	1·6169
75 51, 45 26,	84 44½	1·6410
76 45, 58 20,	86 9	1·7052
76 8, 60 41,	86 0	1·6[illegible]
70 35, 49 15,	84 39	1·6837

The following table of magnetic intensity in different places has been calculated by Hansteen, from Captain Sabine's observations:—

Places.	Latitude	Longitude from Ferro.	Dip.	Intensity.
St Thomas,	0° 25′ N.	24° 25′ E.	0° 6′·1	0·9137
Bahia,	12 59 S.	20 53 W.	4 12	0·8831
Ascension,	7 56 S.	3 16 E.	5 10	0·9922
Maranham,	2 32 S.	26 41 W.	23 6·2	0·9980
Sierra Leona,	8 29 N.	4 25 E.	31 2·5	1·0360
Trinidad,	10 39 N.	43 55 W.	39 2·5	1·1730
St Marys,	13 8 N.	1 7 E.	40 23·1	1·1199
Port Praya,	14 54	7 50 W.	45 26·3	1·1740
Jamaica,	17 56	59 14	46 55·3	1·4077
Cayman,	19 14	63 25	48 48·3	1·4174
Havannah,	23 9	64 43	51 55·2	1·4756
Teneriffe,	28 27	1 25 E.	59 46·8	1·2[illegible]
Madeira,	32 38	0 44	62 12·3	1·3965
New York,	40 43	56 23 W.	73 70·0	1·7863

Ch. XVII.

Places.	Latitude.	Longitude from Ferro.	Dip.	Intensity.
Hammerfest, . .	70° 40′ N.	41° 26′ E.	77° 15′·9	1·4802
Greenland, . . .	74 32	36 30	80 9	1·4990
Spitzbergen, . .	79 50	29 20	81 10·5	1·5373
Davis' Straits, . .	64 0	44 10 W.	83 4	1·6633
Possession Bay, . .	73 31	59 42	86 4	1·6474
B. Martin's Island, .	75 10	86 4	88 26	1·6251
Regent's Island, . .	72 45	72 1	88 27	1·6512
Melville Island, . .	74 27	94 2	88 37	1·6120
Winter Harbour, .	74 47	93 8	88 43	1·5979

This table shows very clearly the want of parallelism in the isodynamic lines.

The following table of intensities has been calculated by Hansteen from his own observations:—

Places.	Latitude North.	Longitude East.	Dip.	Intensity.
Grundsät, . . .	60° 56′	29° 15′	73° 59′·8	1·4336
Björnestad, . . .	61 3	29 8	73 50·1	1·4159
Nebye, . . .	62 18	28 38	74 33·7	1·4172
Rönaas, . . .	62 34	29 15	74 41	1·4339
Ydsät, . . .	62 57	28 58	74 49·1	1·4457
Störens Pfarrhof, .	——	——	74 27·6	1·4142
Drontheim, . . .	63 26	28 5	74 40·7	1·4239
Garneset, . . .	——	——	74 40·4	1·4270
Snul, ...	63 42	29 52	74 38·9	1·4168
Oestersand, . .	63 10	32 12	73 55·9	1·4275
Grimnäs, . . .	62 50	32 50	74 6·8	1·4207
Olsta, ...	62 29	33 40	73 42·6	1·4156
Sundswall, . . .	62 22	34 56	73 37·9	1·4091
Hernösand, . . .	62 38	35 33	73 55·7	1·4146
Docksta, . . .	——	——	74 4·2	1·4054
Umeo, . . .	63 49	37 52	74 4·2	1·4075
Potei, . . .	65 19	39 9	74 9	1·4417
Grötnäs, . . .	——	——	74 58·3	1·4404
Haparandu, near Torneo,	65 50	41 55	75 12·7	1·4384
Alkula, above Torneo, .	66 16	41 27	75 4·5	1·4580
Uleaborg, . . .	65 0	——	74 44·1	1·4333
Brahestad, . . .	64 41	42 20	74 10·4	1·4486
Ny Carleby, . .	63 38	40 31	73 48·2	1·4081
Wasa, . . .	63 4	39 22	73 49·1	1·4100
Tjöck, . . .	62 17	39 2	73 20·9	1·4000
Björneborg, . .	61 29	39 26	72 55·4	1·3935
Obo, ...	60 27	39 58	72 9·9	1·3826
Stockholm, . . .	59 20	35 44	72 8·3	1·3861
Carlstad, . . .	59 23	31 16	72 33·4	1·3717
Kongsvinger, . .	60 12	29 38	73 58·1	1·4136

Part II.

From Hansteen's chart of isodynamic lines it appears, that the line indicating a magnetic intensity of 0·9 is very irregular, but returns upon itself, and encloses a space at about 50° east longitude from Ferro. It is about 2° north of the equator, in the interior of Africa; and its point of greatest southing, is about 48° south latitude, and 35° east longitude from Ferro. It passes as far west as west longitude 39°, in north latitude 10° in Brazil, and goes as far east as the island of Amboyna, in east longitude 142°, and north latitude about 2°. The line marking an intensity of 1·5, goes as far north as latitude 72° beyond the North Cape, but nearly in the same meridian; and it goes as far south as the island of Cuba, passing through the Havannah, in north latitude 23° 9′. The line of intensity 1·8 passes through New York, and has been traced as far north as the bottom of Hudson's Bay, about west longitude 55° from Ferro. But to form a right idea of the position of these isodynamic lines, it would be necessary to inspect the chart of Hansteen, or that of Sabine, given at page 503.

Equator of a sphere.

7. Mr Barlow of Woolwich, in his experimental investigations, in order to discover a method of correcting the deviation of the needle on shipboard occasioned by the iron on board the vessel, discovered that there is a plane passing through every iron sphere, in which, if the centre of a needle be situated, the iron sphere has no effect in altering the declination of the needle. This plane at Woolwich was inclined about 20° to the horizon; it was therefore *perpendicular* to the dip of the needle.* The reason of the non effect of iron upon a needle whose centre is in this plane is sufficiently obvious. The iron must act equally upon both poles of the needle; it cannot therefore have any tendency to alter its declination. When the needle is any where out of that plane, one of its poles being more attracted than the other, it deviates from its true position, and Mr Barlow succeeded in determining the law of this deviation.†

Magnetism proportional to the surface.

8. Another very important general fact, for which we are

* Essay on Magnetic Attractions, p. 6.
† See his Essay on Magnetic Attractions.

indebted to Mr Barlow, is, that the intensity of the action of iron on a magnetic needle, is proportional to its surface, not to its solid contents; so that a hollow sphere of iron, however thin, has as great an effect upon a needle, as a solid iron sphere of the same size.

Other magnetic bodies.

9. It was supposed at first that iron, or substances containing iron, were alone susceptible of the magnetic influence; but it was afterwards observed, that cobalt and nickel are also magnetic metals, and that, like steel, they may be converted into permanent magnets. Coulomb showed, that all bodies were to a certain extent susceptible of magnetic influence.

White hot iron not magnetic.

10. White hot iron has no effect upon the magnetic needle; but as the iron cools it begins to act, and it acts most powerfully at what is called a blood red heat. Mr Barlow and Mr Charles Bonnycastle observed, that between a bright red and a blood red heat, it acts in the opposite way from what it does when cold; that is to say, if it attracts the north pole of the needle when cold, it will attract the south pole at the temperature above specified.*

Needle deflected by motion.

11. Mr Barlow ascertained, that when a magnetic needle is placed at a certain distance from a mass of iron, the needle is deflected when the iron is put into rapid motion. To observe the effects properly, the needle should be neutralized from the action of terrestrial magnetism, by magnets properly placed. When this is attended to, and the iron put in motion, if the direction of the motion be towards the needle, the north pole is deflected about 30° towards the iron; when the motion is from the needle, the north pole is deflected about 30° from the mass of iron. When the needle is in the line of the axis of motion, it suffers no deflection whatever.†

SECTION II.—OF THE ELECTRIC PROPERTIES OF MAGNETS.

It has been already stated that the only metals capable of being converted into permanent magnets, are iron, nickel, and cobalt; but that Œrsted discovered, in the year 1819, that every metal became a magnet during the time that an electric current was passing through it.

* Phil. Trans. 1822, p. 117. † Ibid. 1825, p. 117.

Part II.

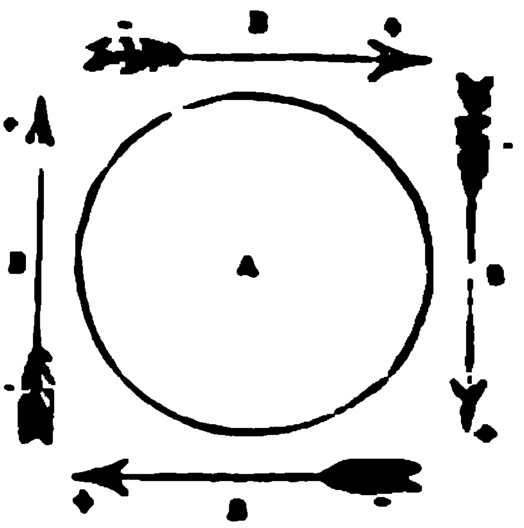
Action of the wire of a galvanic pile on the needle.

He ascertained that if a magnetic needle be presented to the wire which unites the two poles of a galvanic pile in full action, it is attracted by it, and the needle has a tendency to arrange itself at right angles to the conducting wire. If the needle be *above* the conducting wire, (suppose in the magnetic meridian,) the *north* pole turns to the *left* of the current of positive electricity; if it be below the conducting wire, the same pole turns to the *right*. When the needle is placed on the *west* side of the conducting wire, (supposed to be nearly in the magnetic meridian,) and parallel to it, the north pole will be elevated; but when placed on the east side of the conducting wire, the north pole is depressed.

1. The phenomena of electro-magnetism are best observed when only a single pair of plates is used, and the larger the size of the plates, the better can the phenomena be observed. A good apparatus for the purpose, consists of a box of copper, composed of two copper plates, each about a foot square, and about ¾ths of an inch distant. A bottom and ends of copper are soldered to their sides, so as to complete the box. In the centre, between the two sides, and not touching the copper anywhere, a zinc plate of nearly the same size as the copper, is fixed either by baked wood or sealing-wax. When this vessel is filled with dilute nitric acid, and the copper and zinc plates are connected by a wire, most of the phenomena of electro-magnetism may be exhibited by means of this wire.

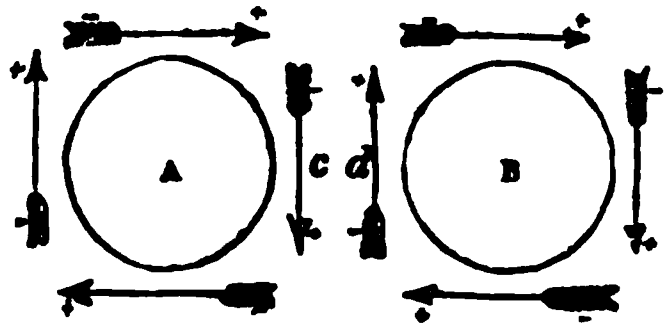
Action on the needle explained.

2. To form a conception of the cause of this difference in the declination and inclination of the needle according to its position, we may suppose that the current of electricity moves in a spiral direction round the conducting wire, moving from left to right. This will be understood by inspecting the figure in the margin, in which A represents a section of the conducting wire, and the arrows B, B, B, B represent the direction in which the current of electricity moves round A. It is obvious from the position of these arrows, that the action on the magnetic needle ought to be opposite above and below, and likewise upon the left and right of the conducting wire.



3. M. Ampere, to whom we are indebted for an investigation of the phenomena of electro-magnetism conducted with much sagacity, discovered that when two electric currents are moving in the same direction in conducting wires parallel to each other they *attract*, but when moving in opposite directions they repel. He contrived to place, parallel to each other, two conducting wires, uniting the negative and positive poles of a galvanic battery. One of these conducting wires was moveable, and the consequence was, that it was attracted by, and made to approach the immovable conducting wire. When the currents of electricity were moving contrary ways, the moveable conducting wire was repelled, and receded from the immovable. As similar electricities repel and dissimilar attract, this phenomenon may appear, at first sight, inconsistent with the laws of electricity. But the simple inspection of the figure in the margin will serve to render it intelligible. A and B are sections of the two conducting wires; and the arrows indicate the way in which the electric currents move along them in a spiral direction. It is obvious from the directions of the arrows *c* and *d*, that negative and positive electricity are directly opposite to each other, and that, of course, the wires ought to attract each other as they are found to do. This way of accounting for the phenomena of electro-magnetism was first employed by Œrsted. It was afterwards used by others; particularly by Dr Wollaston and M. Ampere, with much felicity.

Electric currents moving the same way attract.



M. Arago and Sir Humphrey Davy observed about the same time that the conducting wire, while the pile is in activity, attracts iron filings; but the moment the contact is interrupted, the filings drop off. But bodies not magnetic are not attracted by it; showing that the phenomenon cannot be explained upon the common principles of electrical attraction.

4. Since iron, nickel, and cobalt, alone are capable of constituting permanent magnets, though every other metal is a magnet as long as a current of electricity is passing through it, we cannot doubt, that in every permanent magnet, a current

Electricity makes all metals magnetic.

Part II. of electricity is passing, as long as it retains its magnetic virtue. There must be something, therefore, in the constitution of iron, nickel, and cobalt, different from that of other metals. Their elements must be capable of such an arrangement, as to resemble that of a Voltaic pile. It is well known that soft iron cannot be converted into a permanent magnet; it must be combined with a certain quantity of carbon, sulphur, or phosphorus. Is it not probable that the iron and carbon, sulphur, or phosphorus correspond with the copper and zinc plates in the Voltaic battery? As no chemical decomposition takes places, we cannot explain how the electricity is evolved; but that it is evolved, and that a constant current of electricity is passing through every permanent magnet, has been demonstrated by satisfactory experiments. A good many years ago sparks were obtained from the magnet, I believe first by Mr Faraday, in the year 1831. They were afterwards obtained by Messrs. Nobili and Antinori,* who were induced to experiment on the subject, from hearing of the success of Faraday. They contrived an apparatus, by means of which sparks might be obtained from the magnet at pleasure. In the year 1832, sparks were obtained by Professor Forbes of Edinburgh, before he was aware of the method practised by Nobili and Antinori.† In 1832, the apparatus was still farther improved by the sons of M. Pixii, philosophical instrument-maker, in Paris.‡

It consists of a large horse-shoe magnet. At the extremity of each pole, not absolutely in contact but as near as possible, is placed a piece of soft iron, nearly of the size of the pole of the magnet, but its section is circular, instead of being a rectangle, like the extremities of the magnet. A copper wire, covered with silk thread, is wrapt, in the form of a helix, round each piece of soft iron, so as to make a great many revolutions round each. The extremities of these wires dip into a little cavity placed before the apparatus, and attached to it, containing mercury; or rather one dips into the mercury, while the other is attached to a small wheel, composed of two

* Ann. de Chim. et de Phys. xlviii. 417.

† Phil. Mag. (third series,) i. 49.

‡ Ann. de Chim. et de Phys. l. 322.

pieces of iron wire crossing at right angles, and attached to the axis of the horse-shoe magnet, and moving round when that axis is set in motion. At every revolution of the axis, the extremities of the iron wires dip four times into the mercury, so that the contact is renewed and broken four times successively at each revolution. The axis of the horse-shoe magnet is fixed horizontally, and connected by a strap to a wheel which can be turned rapidly by a handle, causing the horse-shoe magnet to revolve rapidly about its axis.

The pieces of soft iron surrounded by the helices of copper wire being fixed, when the magnet revolves, its two poles come alternately in contact with each piece during each revolution. It is well known that a piece of soft iron, when in the neighbourhood of a magnetic pole, acquires the same magnetic properties as that pole. But as soon as it leaves the proximity of that pole it loses its magnetism, and acquires the opposite state when it gets into the neighbourhood of the other pole. Thus the magnetic state of each piece of soft iron, and consequently of the surrounding helices, changes with each revolution. This causes a perpetual interruption and renovation of the circuit: hence the sparks, which appear only when a current is interrupted or renewed.

By this apparatus constant sparks may be obtained, fine platinum wire may be ignited, and various chemical phenomena may be produced.

5. When an electric current passes through a wire covered with silk thread, the wire becomes a magnet. If this wire is twisted round, in the form of a helix, every revolution constitutes a distinct magnet having two poles. As these poles are always at the same extremities of the portion of the wire, it is obvious that the force exerted by this helix must increase in proportion to the number of turns which it makes: hence the theory of the Galvano-multiplier of Schweigger, with all its subsequent improvements.

6. It has been shown by the experiments of Biot and Savart, that when an electric current passes near a magnetic needle, the action of the current on the needle is inversely as the distance. They have shown also, that if the elementary force be

Part II. inversely as the square of the distance, then the action of long wire will be inversely as the distance.*

CHAPTER XVIII.

OF DECOMPOSITIONS BY WEAK ELECTRIC CURRENT

It has been long known that certain metals have the proper of precipitating other metals from their solutions in the metali state. Thus, if a plate of copper be put into a solution c nitrate of silver, the copper dissolves while the silver is pre cipitated in the metallic state. Mercury equally throws dow silver from the nitrate. Lead or tin throws down copper: an zinc or iron is capable of throwing down lead and tin. M Sylvester first showed, by some ingenious experiments i 1806,† that these precipitations were produced by electri currents; that water was decomposed by these currents; an that the hydrogen evolved united with the oxygen of th metallic oxide, and thus caused the metal to separate in th metallic state.

Ritter had already observed, in 1800, that these electric effects are still more sensible when the circuit is made b means of two liquids and a metal.‡ In 1807, Bucholz§ ma some experiments on these feeble circuits, which deserve t be noticed. He put into a cylindrical glass vessel, a soluti of 1 part of green chloride of copper in 4 parts of water; ove this he poured, cautiously, 6 parts of water, so that the wat floated on the surface of the chloride. A polished plate c copper, half an inch broad and six inches in length, was ma to pass through the two liquids. In two hours the copp plate was covered with a white matter, which was dichlori of copper. He repeated his experiments, substituting a c

* Becquerel, ii. 432.

† Nicholson's Jour. xiv. 9[illegible]

‡ Histoire de Galvanism par Sue aîné, ii 116

§ Gehlen's Journal, (second series,. v. 127.

centrated solution of nitrate of copper, for the chloride. A portion of the copper plate at the line of separation of the two liquids remained brilliant and untarnished. The portion of copper above this line was covered with black oxide; the under portion of the plate was covered with small grains of metallic copper. The result was the same when the water was acidulated with nitric acid.

When nitrate of silver was substituted for nitrate of copper, and a plate of silver for that of copper, the result was the same. Metallic silver in minute cubes was deposited upon the under portion of the silver plate, and oxide of silver upon the upper portion. Similar experiments were made with nitrate of lead and a plate of lead, chloride of zinc and a plate of zinc, sulphate of iron and a plate of iron, with similar results.

Precipitation of metals on metallic plates.

In 1826 the subject was taken up by M. Becquerel,* to whom we are indebted for a very complete investigation of this interesting subject, and for applying it to explain the numerous changes which are continually occurring in the metallic veins, and even the rocks constituting the surface of the earth. Into a glass tube, bent in the form of a U, he poured a solution of sulphate or nitrate of copper; a copper wire was plunged into each leg of the tube, and these wires were brought into connexion with a small thermo-electric apparatus. The copper wire connected with the negative pole became covered with metallic copper, while the other extremity was oxydized. Solutions of tin, zinc, silver, and lead, with wires of the same metals gave similar results. Wires of platinum and gold in the same solutions produced no effect; the case was different when platinum or gold wire was made to act on nitrate of silver.

From these facts he concluded, that by very feeble electric forces, metals easily reducible are precipitated from their solutions, by plates of the same metal as that held in solution. Becquerel, in these first experiments, showed that decompositions of metallic solutions are produced most easily by the action of metals the same with those held in solution. He

* Ann. de Chim. et de Phys. xxxiv. 152.

 gave a description of a small apparatus, capable of continuing active for months, by means of which these decompositions may be easily induced.

It consists of two glasses, the one containing nitric acid and the other a solution of potash. Into each of them is plunged a plate of platinum, to which a wire is united; and the communication between the two liquids is established by means of a bent glass tube filled with clay, moistened with an aqueous solution of common salt. The reaction of the acid and alkaline solutions on that of the common salt, evolves enough of electricity to produce decompositions. M. Becquerel, in analysing these phenomena, discovered that oxygen is much more easily transferable by electric currents than sulphuric acid.

Formation of chlorides and iodides.

M. Becquerel prosecuted these interesting investigations with assiduity, and in 1827, his experiments on the formation of chlorides and iodides by weak electric currents, were read to the Academy of Sciences of Paris, and published during the same year.* He found that when a neutral salt is substituted for a metallic salt, these feeble electric currents cannot traverse the liquid when we employ wires of gold or platinum to complete the current. With a wire of silver the current is very feeble; but with wires of zinc, lead, iron, and tin, the effects are well marked. This difference depends upon the easy alteration of the oxidable metals, an alteration which facilitates the passage of the electricity of a wire into a solution.

Of double chlorides.

It had been previously ascertained that chlorides have a tendency to combine with each other so as to constitute double chlorides.† Becquerel attempted to form these double chlorides by means of a weak electrical current. The difficulty consists in employing the requisite electrical force. If the intensity is too great, we isolate all the elements; if it is too feeble we remove only one, two, or three of these elements.

* Ann. de Chim. et de Phys. xxxv. 126.

† Some chlorides, as has been shown by Mr Bonsdorf, have the characters of an acid, and others those of a base: hence they combine and form compounds analogous to the salts formed by the union of oxygen acids with oxygen bases.

He put a plug of amianthus into the bottom of a glass tube bent into the shape of a U. Into one of the branches he put a solution of sulphate of copper, and into the other a solution of common salt; then the communication between the two liquids was completed by a plate of copper. In a short time that end of the plate which was plunged into the sulphate of copper, became covered with metallic copper; on the other extremity of the plate were deposited octahedral crystals of the double chloride of sodium and copper, which are decomposed by the contact of water. Similar double chlorides were obtained when solutions of silver and of other metals were substituted for the sulphate of copper. When sal ammoniac was employed instead of common salt, there were obtained the double chlorides of ammonium and silver, ammonium and lead, &c.

Double iodides.

The double iodides were formed in the same way. M. Becquerel put into one branch of the tube a solution of sulphate of copper, into the other, of hydriodate of potash. Copper in the metallic state was precipitated, as before, on the end of the lead plate plunged into the copper solution; while, upon the other extremity of the lead plate, long silky filaments appeared, consisting of the double iodide of potassium and lead.

He took a tube shut at one of its extremities, put into the bottom of it any oxide—the black oxide of copper, for example—then he poured over it a solution of nitrate of copper, and plunged into it a plate of copper, descending to the very bottom of the tube. In a fortnight there were deposited upon the plate of copper, above the oxide, red octahedral crystals of suboxide of copper. With the subacetate of lead, a plate of lead and litharge, we obtain the protoxide of lead in pentagonal dodecahedrons, or in silky filaments.

Sulphurets, &c.

In 1829, M. Becquerel prosecuted these experimental investigations, and succeeded in forming sulphurets, iodides, and bromides, by similar processes.* He had conceived the idea, that certain compounds had been formed by means of succes-

* Ann. de Chim. et de Phys. xlii. 225.

Part II. sive decompositions; and this idea was verified during these investigations. To obtain sulphuret of silver, such as it occurs in nature, he employed a glass tube, curved in the form of a U, having a quantity of moist clay at the curvation, in order to keep the two liquids employed distinct. Into one of the legs of the tube he poured a solution of hyposulphite of potash, and into the other a solution of nitrate of silver; and then established a communication between the upper portion of the two liquids, by means of a silver wire. A very weak electric circuit was formed; the extremity of the silver wire, plunged into the nitrate, became the negative pole, and the other extremity the positive pole. The nitrate of silver was decomposed, and the silver was precipitated in the metallic state. The oxygen and nitric acid being transferred to the other pole, determined the formation of a double hyposulphite, the first by oxydizing the silver, and the second by decomposing the hyposulphite; the consequence was the formation of a double hyposulphite in fine crystals. The nitric acid still continuing to come to that pole, acted upon the double hyposulphite, decomposed it, and occasioned the formation of very regular crystals of sulphuret of silver: hence the crystallization of the sulphuret is owing to the slow decomposition of the double hyposulphite.

The energy of this apparatus being very feeble, the silver deposited at the negative pole has time to assume a regular crystalline form. By proceeding in the same manner, crystals of all the other metals may be obtained.

If for silver and its nitrate, we substitute copper and its nitrate, we form a double hyposulphite of copper and potash, which crystallizes in very fine silky needles. By continuing the action, this double salt is decomposed, and crystals of sulphuret of copper with triangular faces are obtained. By a similar process the oxysulphuret of antimony may be formed. He obtained, likewise, sulphurets of tin, lead, mercury, iron, and zinc.

Iodides. M. Becquerel followed the same process in order to obtain the metallic iodides, the crystals were very distinct. He substituted hydriodate of potash or soda for the alkaline hyposul-

phite. With lead he obtained first a double iodide of lead and potassium, which crystallized in very fine white silky needles. By little and little that compound was decomposed, beginning at the under part contiguous to the clay. A great number of octahedral crystals were observed, of a golden yellow colour and a brilliant appearance. These crystals constituted iodide of lead. Copper subjected to the same treatment gave, at first, a double iodide in white needles; this being decomposed in its turn, beautiful crystals of iodide of copper were formed. Ch.XVIII

M. Becquerel carried these successive decompositions and crystallizations much farther, in a paper read to the French Academy, in January, 1830.* It had been shown by Davy, that when various solutions are mixed together and exposed to electric currents, the acids and oxygen pass to the positive pole, while the bases and hydrogen make their way to the negative pole. The same thing happens when the solutions are put into two capsules, communicating with each other by moistened amianthus. This always takes place when the electrical force is great enough to produce decomposition in the solutions; but if it be strong enough to decompose one only, then the elements of this one are transported into the other capsule, where they generally produce modifications which lead to the formation of new compounds.

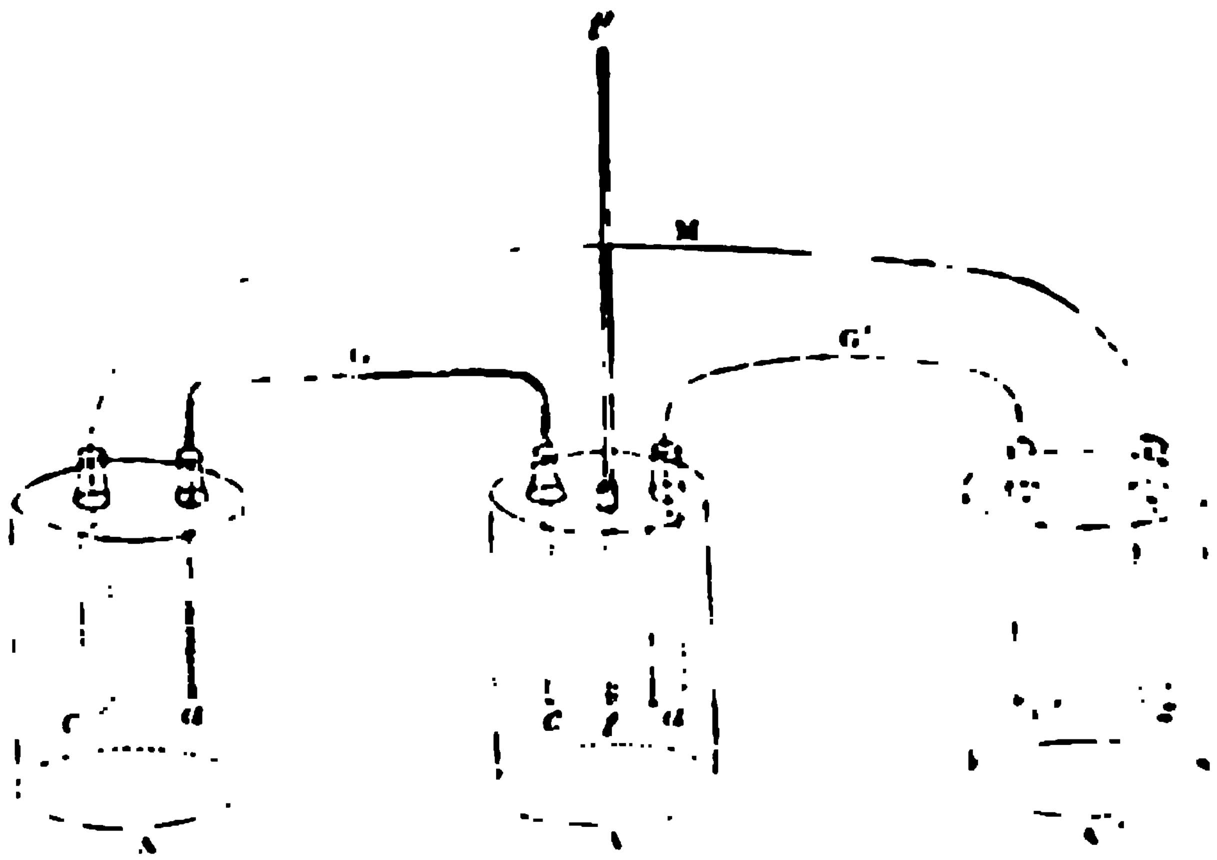
Double effects.

M. Becquerel in this paper examines, in the first place, the case when the metal found at the positive pole of a solution, concurs by the reaction of its oxide to the formation of compounds. He took two glass vessels; into the one he poured a solution of sulphate of copper, and into the other an alcoholic solution of sulpho-carbonate of potash. The communication was established between these glasses by means of an inverted glass syphon, filled with clay moistened with a solution of nitrate of potash, and by means of an arc composed of two plates of copper and lead—the copper dipping into the sulphate, and the lead into the sulpho-carbonate. An electrical current is produced by this arrangement, which acts in the following manner:—Copper is precipitated in the glass con-

* Ann. de Chim. et de Phys. xliii. 131.

taining the sulphate. Bicarbonate of potash is deposited in crystals on the sides of the vessel containing the sulpho-carbonate. A double sulpho-carbonate of lead and potash is deposited in the same vessel, in acicular crystals, together with carbonate of lead, and probably sulphate of potash, and sulphate of lead. Finally, octahedral crystals of sulphur with rhomboidal bases, were also deposited. In this experiment, the property which sulphuric acid possesses of separating the weaker acids from the bases with which they were united, exerts its force only in consequence of the feebleness of the electric current. Were its energy greater, all the acids, without distinction, would be transferred to the positive pole.

M. Becquerel gives a description of an apparatus which allows the experimenter to avoid or employ at pleasure the reaction of metallic oxides. It may be worth while to give the reader an idea of this apparatus:—It consists of three glass



cylindrical vessels, A A′ A″, placed near each other upon the same horizontal plane. The first is filled with a solution of sulphate or nitrate of copper; the second, with a solution of the substance on the constituents of which we wish to operate, and the third, with water rendered slightly conducting by the addition of an acid, or of common salt. A communicates with A′ by means of a bent tube, a G c, filled with clay m[illegible]

tened with a saline solution, the nature of which depends upon the effect which we wish to produce in A′. A′ and A″ communicate by means of a plate of platinum or gold, *a′* G′ *c′*. And finally, A and A″ are united by two metals composed of two plates, M*c* and M*z*, of copper and zinc. Finally, a tube of safety, *t t′*, is placed in the glass vessel A′, to indicate the pressure produced by the evolution of gas. From this disposition, the extremity *a′* of the plate of platinum is the positive pole of a small electric pile, the action of which is slow, but constant. When the liquid contained in A′ is a good conductor, the intensity of the current is sufficient to decompose the sulphate of copper in A. When the oxygen passes to *a′*, as well as the sulphuric acid, which in passing into the tube *a* G *c*, disengages sometimes the acids which have less affinity than it has for the bases with which they are combined, all the elements pass into the liquid A′, where their slow action on each other occasion different charges.

Sometimes it is necessary to place a fourth glass cylinder between A and A′. Into it a sufficient quantity of the saline solution to be decomposed by the sulphuric acid is put, so that the effects produced in the liquid A′ may not be interrupted when all the salt in the moist clay is decomposed. In this case, when we wish to transfer an electro-negative gas, or a nascent acid, into the liquid in A′, it will be sufficient to place in the clay a solution, which by its reaction on the sulphuric acid coming from the decomposition of the sulphate of copper, allows that gas, or that acid, to be disengaged. But if the object be to transport hydrogen, or an electro-positive gas, we must reverse the means of communication, and put *a′* G′ *c′* in place of the tube *a* G *c*. Finally, if we replace the platinum plate by a plate of an oxydisable metal, we introduce into the solution the reaction of an oxide, which being in a nascent state, concurs in the formation of the products.

An example or two will serve to render the use of this apparatus familiar. Into the glass cylinder A′ was put an alcoholic solution of sulpho-carbonate of potash, into the glass A, a solution of sulphate of copper, and into the clay of the

Part II.

tube a G c, a solution of nitrate of potash. In twenty-four hours the reaction of the oxygen, and of the nitric acid, on the solution of the sulpho-carbonate of potash was sensible; for upon the platinum plate were observed crystals of sulphur and of bicarbonate of potash.

When we substitute for the alcoholic solution of sulpho-carbonate of potash, an aqueous solution of sulpho-carbonate of barytes, analogous reactions become sensible; sulphur is precipitated in small crystals, and sulphate of barytes crystallizes in prismatic needles.

Crystals of carbonate of lime obtained.

In 1831, M. Becquerel discovered a method of obtaining artificial crystals of carbonate of lime.* Cruikshanks and Daniell had observed that when we expose to the air a solution of sugar and lime, in a large open vessel, the surface becomes covered with small crystals, which fall to the bottom of the vessel, and are speedily replaced by others. These were crystals of carbonate of lime; but the mode of their formation was disputed. M. Becquerel took a glass tube bent into the form of a U, at the curvature of which a plug of cotton was placed to separate the two liquids from each other. Into the legs of this syphon, he poured a solution of 1 part of lime, and 16 parts of sugar, in 100 parts of water. Into each leg of the syphon was plunged a plate of platinum, communicating with the poles of a Voltaic battery, charged with a weak solution of common salt, containing one per cent. of sulphuric acid. Finally, all the openings of the tubes were carefully shut in order to exclude the common air. The reactions were such that carbonate of lime, partly in crystals, was precipitated upon the positive plate: hence it is clear, that the formation of the carbonate must have been owing to the decomposition of the sugar; but the subject was afterwards examined by M. Pelouze, who showed that the only use of the sugar, in the experiments of Cruikshanks and Daniell, was to increase the solubility of the lime, and that the carbonic acid of the carbonate came from the atmosphere.

M. Becquerel was induced by these experiments to invest

* Ann. de Chim. et de Phys. xlvi. 5

Action of sugar on oxide of copper.

gate the action of sugar and mucilage on metallic oxides, by the intervention of alkalies and earths;* and we may notice his results though the subject is not connected with electric currents. If we mix oxide of copper, newly precipitated, with lime and water, and apply heat, the oxide becomes black, being rendered anhydrous. If we add a little common sugar, a portion of the oxide dissolves, and the liquid assumes a blue colour, similar to that of a solution of oxide of copper in ammonia. Honey and sugar of milk have the same property. Potash and soda behave precisely as lime does, excepting only that their dissolving power is greater. Barytes and strontian possess that property only feebly. Thus, potash, soda, and lime, have the property of dissolving oxide of copper, even without heat, when any sugar exists in the solution; a character which may enable us to recognise the presence of sugar in solutions of organized substances. Gum possesses no such property. Gum has the power of precipitating gum when added to a solution of gum containing an alkali or an earth.

Reduction of acids.

Experience having proved that the affinity of a body for another greatly facilitates electro-chemical decompositions, Becquerel employed this principle to reduce oxide of iron, magnesia, zirconia, and glucina.† He conceived from reflecting on the chemical reactions which take place in living animals, that vitality developes peculiar electric forces, which, though feeble in appearance, produce nevertheless effects which can only be obtained by affinities of a certain energy. One of the methods of giving probability to this opinion, is to prove that we can produce in inorganic compounds, energetic chemical effects, with feeble electric forces. He took two glass tubes, containing each in its bottom a quantity of pure clay, and plunged half way into a vessel filled with water. Into the first he poured a solution of nitrate of copper, and by a plate of platinum connected it with the positive pole of a Voltaic trough of thirty pairs. Into the second tube, connected in the same way with the negative pole, was put a

* Ann. de Chim. et de Phys. xlvii. 13. † Ibid. xlviii. 337.

Part II. solution of common salt. In this case water alone was decomposed, and not nitrate of copper. The reason obviously was that the oxide of copper could not be transported through the pure water, because it could not form in that liquid a soluble compound. If we operate in the inverse manner, that is, if we put the nitrate of copper into the negative tube, decomposition takes place; the copper being deposited upon the negative pole, and the acid transported through the liquid to the positive pole.

If we pour into a negative tube a solution of sulphate of copper, and into the positive tube a solution of nitrate of potash, the nitric acid is disengaged in this last tube; the potash in passing to the negative tube acts on the sulphate of copper, separating a portion of the oxide of copper, which is reduced, and occasions the double sulphate of copper and potash which crystallizes. As long as sulphate of copper remains to be decomposed, and as there is a sufficient quantity of nitrate of potash in the positive tube, the sulphuric acid is not transported into that tube; but as soon as the double sulphate begins to be decomposed, its presence there may be rendered sensible by nitrate of barytes. We may lay it down as a general rule, that when two salts are in solution, the most soluble is always decomposed in preference of the other.

It was supposed at first that pretty strong electric currents were necessary to decompose solutions of manganese, zinc, iron, tin, and arsenic, and to obtain a certain quantity of the reduced metal at the negative pole. The solutions of titanium, nickel, cobalt, uranium, and chromium, offered at first no sensible traces of decomposition; but M. Becquerel soon discovered that the presence of water was an obstacle to the decomposition of these salts, especially when the oxygen of the water has less affinity for the hydrogen than it has for the metal. When this was the case, water alone might be decomposed. On this account it was necessary to employ very concentrated solutions, or sometimes the oxides themselves slightly moistened.

Even these precautions are not sufficient when we wish to reduce refractory oxides. We must then have recourse

another method. When the hydrogen gas arrives at the negative pole, it must exert there a reducing power so much the more energetic the longer it remains in the state of nascent gas, a state the most favourable possible to chemical action. The less rapid the current of gas disengaged is, the longer will it remain in the nascent state. But this requires a weak electric current: hence a certain degree of electric force will be more successful in reducing these refractory metals, than a stronger one would be.

Guided by these theoretical considerations, Becquerel reduced from these solutions, and with an exceedingly weak force, iron, zirconium, glucinum, and magnesium, in crystals or crystalline plates.

To reduce iron to the metallic state, he employed a battery of five or six pairs, feebly charged, or even of a single pair. He made use of two tubes arranged as formerly described. Into the negative tube he poured a solution of the protosulphate of iron, and into the other a solution of common salt; and the battery was so weakly charged that the evolution of gas was scarcely sensible. The soda and hydrogen passed into the negative tube. The alkali uniting with the sulphuric acid, formed a double sulphate of iron and soda; while the hydrogen reacting on the oxide at the instant of its precipitation, occasioned its reduction. The iron is deposited upon the negative plate, sometimes in very small crystals, sometimes as if it had been in fusion, (especially when the action is very slow,) and sometimes in small round tubercles more or less crystalline. When the plate is covered with crystals symmetrically arranged, it possesses magnetic polarity, owing doubtless to the want of continuity in the particles of iron; for the permanency of the magnetic properties of steel is probably owing to the interposition of the particles of carbon, which prevents the two kinds of magnetism from combining together after having been separated.

Magnesia is reduced with more difficulty than iron. Becquerel was obliged to employ a battery of thirty pairs of plates. He put into the negative tube chloride of magnesium; which having been formed in a silver vessel, contained a small

Part II. quantity of that metal, which was favourable to the success of the process. There was first formed upon the platinum plate a grey deposite, then tubercles of the same colour, and finally, octahedral crystals of a silver-white colour, possessing all the characters of magnesium.

Cementation explained. M. Becquerel employed electric currents to explain how iron is converted into steel by the process of *cementation*, and how in many rocks the elements are transferred from within to without, and replaced by others without any loss of solidity being the consequence. Nature must employ forces capable of transporting these elements in two different directions, and to great distances. Now, electric currents supply all the forces necessary to produce such effects.

When iron and charcoal are in contact at the temperature of ignition, if we bring both of these bodies in contact with an electro-multiplier, we shall find that a strong electric current exists, which passes from the iron into the charcoal—the same direction as that of the current produced by the combination of iron with oxygen, or of a base with an acid. By means of this current, the particles of carbon are transferred, at a red heat, into the interior of the iron, and thus the iron becomes steel.

Crystals of oxides obtained. In 1832, M. Becquerel showed how, by means of electric currents, crystals of metallic oxides might be obtained.* To dissolve the oxides he employed peroxide of potassium, which, in consequence of its high degree of oxidation, cannot be capable of forming with these oxides combinations so stable as those with potash, which is a more energetic base.

When potash is melted in an open silver crucible, it loses a part of its water, and absorbs oxygen from the atmosphere, forming peroxide of potash, which is decomposed by water, oxygen gas being disengaged. If we put into a silver crucible half a gramme of black oxide of copper, with two or three grammes of caustic potash, prepared by means of alcohol, and keep its temperature for a few minutes at incipient ignition, the oxide of copper will be completely dissolved. When the

* Ann. de Chim. et de Phys. li. 101.

solution cools, oxygen gas is disengaged, flocks of black oxide of copper are disengaged, and small crystals of anhydrous black oxide are formed, some of which have sometimes a length of several millimeters.

When the crucible is kept for some time in a red heat, the black oxide of copper is changed into red oxide, which is obtained in small crystals of a brick-red colour.

When protoxide of lead is substituted for black oxide of copper, we obtain that protoxide crystallized in small cubes or square plates, provided the crucible be kept only a very short time at an incipient red heat. If the red heat be continued for some time, the protoxide of lead passes into the peroxide, which crystallizes in hexagonal plates of a flea-brown colour. Phosphate and sulphate of lead treated in the same way are completely decomposed, while crystals of protoxide and peroxide of lead are formed. By the same process the oxide of cobalt may be obtained in square plates, and the oxide of zinc in needles of a dirty yellow colour.

It has been often remarked, that in chemical operations the walls of the vessel have an influence on the reactions. M. Becquerel attempted to ascertain the nature of this influence.* He took a narrow glass tube, open at both extremities, and introduced by one end a quantity of calcined oxide of cobalt reduced with water into a very fine paste. The aperture was then closed, a silver wire introduced into the tube, and the upper portion of it filled with a solution of chloride of chromium. After a fortnight small dendritical metallic crystals began to be perceived on the inferior part, and on the surface of the tube. He obtained the same effect without employing the silver wire. The oxide of cobalt, which is not easily reduced by the action of Voltaic electricity, yields in this instance to a current apparently feeble, but which, however, must possess considerable energy.

The inner surface of the tube must have exercised a great influence, as the reduction took place above. In the slow action of the chloride of chromium on the oxide of cobalt,

* Ann. de Chim. et de Phys. lii. 243.

Part II. electricity was disengaged. The two fluids must have unite along the inner surface of the tube, which served to establis the poles of the small battery.

M. Becquerel's next attempt was to obtain sulphurets b other processes.* He experimented chiefly on sulphurets o lead and of antimony. Into a tube of glass, shut at one en he put a quantity of sulphuret of mercury; over it was poure a solution of chloride of magnesium, and a metallic plate o lead was passed through the solution to the very bottom o the tube. The tube was now sealed hermetically, and it contents left to the slow action of the electric current induce by their mutual action on each other. In about six weeks thin metallic film began to be perceived on the inner surfac of the tube, just above the sulphuret of mercury. This fil detached itself, and its inner side was studded with tetra hedral crystals of pure sulphuret of lead. On opening th tube, a gas was disengaged, having the odour belonging t combinations of sulphur with chlorine and hydrogen. Anti mony subjected to a similar treatment, also furnished crystals but their nature was not so accurately determined, thoug probably they were sulphuret of antimony.

Such are the most important of the experiments made b M. Becquerel to form a great number of combinations an decompositions by means of weak electric currents. At th meeting of the British Association held at Bristol in 1836 Andrew Crosse, Esq., read to the chemical section the resul of a set of similar experiments, on which he had been engage for several years. Without being aware of what had bee previously done by M. Becquerel, Mr Crosse had obtaine many similar results, and came to similar conclusions. I will be sufficient if we state here the general result of Mr Crosse's investigations.

Experiments of Mr Crosse. In the neighbourhood of Broomfield, in Somersetshire there is a cavern, the roof of which is partly studded with arragonite and calcareous spar in fine crystals. The water which flows from that roof contains in the quart about ten

* Ann. de Chim. et de Phys. liii. 105.

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xis of the horse-shoe magnet, and moving round when axis is set in motion. At every revolution of the axis, xtremities of the iron wires dip four times into the mercury, at the contact is renewed and broken four times succesy at each revolution. The axis of the horse-shoe magnet ced horizontally, and connected by a strap to a wheel h can be turned rapidly by a handle, causing the horsemagnet to revolve rapidly about its axis.

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OF DECOMPOSITIONS BY WEAK ELECTRIC CURREN

It has been long known that certain metals have the prop of precipitating other metals from their solutions in the met state. Thus, if a plate of copper be put into a solutio nitrate of silver, the copper dissolves while the silver is p cipitated in the metallic state. Mercury equally throws d silver from the nitrate. Lead or tin throws down copper; zinc or iron is capable of throwing down lead and tin. Sylvester first showed, by some ingenious experiment 1806,† that these precipitations were produced by elec currents; that water was decomposed by these currents; that the hydrogen evolved united with the oxygen of metallic oxide, and thus caused the metal to separate in metallic state.

Ritter had already observed, in 1800, that these electr effects are still more sensible when the circuit is made means of two liquids and a metal.‡ In 1807, Bucholz§ m some experiments on these feeble circuits, which deserve be noticed. He put into a cylindrical glass vessel, a solut of 1 part of green chloride of copper in 4 parts of water; o this he poured, cautiously, 6 parts of water, so that the wa floated on the surface of the chloride. A polished plate copper, half an inch broad and six inches in length, was m to pass through the two liquids. In two hours the cop plate was covered with a white matter, which was dichlo of copper. He repeated his experiments, substituting a

* Becquerel, ii. 432. † Nicholson's Jour. xiv. 9..

‡ Histoire de Galvanism par Sue ainé, ii. 116.

§ Gehlen's Journal, (second series,) v. 127.

centrated solution of nitrate of copper, for the chloride. A portion of the copper plate at the line of separation of the two liquids remained brilliant and untarnished. The portion of copper above this line was covered with black oxide; the under portion of the plate was covered with small grains of metallic copper. The result was the same when the water was acidulated with nitric acid.

When nitrate of silver was substituted for nitrate of copper, and a plate of silver for that of copper, the result was the same. Metallic silver in minute cubes was deposited upon the under portion of the silver plate, and oxide of silver upon the upper portion. Similar experiments were made with nitrate of lead and a plate of lead, chloride of zinc and a plate of zinc, sulphate of iron and a plate of iron, with similar results.

Precipitation of metals on metallic plates.

In 1826 the subject was taken up by M. Becquerel,* to whom we are indebted for a very complete investigation of this interesting subject, and for applying it to explain the numerous changes which are continually occurring in the metallic veins, and even the rocks constituting the surface of the earth. Into a glass tube, bent in the form of a U, he poured a solution of sulphate or nitrate of copper; a copper wire was plunged into each leg of the tube, and these wires were brought into connexion with a small thermo-electric apparatus. The copper wire connected with the negative pole became covered with metallic copper, while the other extremity was oxydized. Solutions of tin, zinc, silver, and lead, with wires of the same metals gave similar results. Wires of platinum and gold in the same solutions produced no effect; the case was different when platinum or gold wire was made to act on nitrate of silver.

From these facts he concluded, that by very feeble electric forces, metals easily reducible are precipitated from their solutions, by plates of the same metal as that held in solution. Becquerel, in these first experiments, showed that decompositions of metallic solutions are produced most easily by the action of metals the same with those held in solution. He

* Ann. de Chim. et de Phys. xxxiv. 152.

Part II.

gave a description of a small apparatus, capable of continuing active for months, by means of which these decompositions may be easily induced.

It consists of two glasses, the one containing nitric acid and the other a solution of potash. Into each of them is plunged a plate of platinum, to which a wire is united; and the communication between the two liquids is established by means of a bent glass tube filled with clay, moistened with an aqueous solution of common salt. The reaction of the acid and alkaline solutions on that of the common salt, evolves enough of electricity to produce decompositions. M. Becquerel, in analysing these phenomena, discovered that oxygen is much more easily transferable by electric currents than sulphuric acid.

Formation of chlorides and iodides.

M. Becquerel prosecuted these interesting investigations with assiduity, and in 1827, his experiments on the formation of chlorides and iodides by weak electric currents, were read to the Academy of Sciences of Paris, and published during the same year.* He found that when a neutral salt is substituted for a metallic salt, these feeble electric currents cannot traverse the liquid when we employ wires of gold or platinum to complete the current. With a wire of silver the current is very feeble; but with wires of zinc, lead, iron, and tin, the effects are well marked. This difference depends upon the easy alteration of the oxidable metals, an alteration which facilitates the passage of the electricity of a wire into a solution.

Of double chlorides.

It had been previously ascertained that chlorides have a tendency to combine with each other so as to constitute double chlorides.† Becquerel attempted to form these double chlorides by means of a weak electrical current. The difficulty consists in employing the requisite electrical force. If the intensity is too great, we isolate all the elements; if it is too feeble we remove only one, two, or three of these elements.

* Ann. de Chim. et de Phys. xxxv. 126.

† Some chlorides, as has been shown by Mr Bonsdorf, have the characters of an acid, and others those of a base: hence they combine and form compounds analogous to the salts formed by the union of oxygen acids with oxygen bases.

He put a plug of amianthus into the bottom of a glass tube bent into the shape of a U. Into one of the branches he put a solution of sulphate of copper, and into the other a solution of common salt; then the communication between the two liquids was completed by a plate of copper. In a short time that end of the plate which was plunged into the sulphate of copper, became covered with metallic copper; on the other extremity of the plate were deposited octahedral crystals of the double chloride of sodium and copper, which are decomposed by the contact of water. Similar double chlorides were obtained when solutions of silver and of other metals were substituted for the sulphate of copper. When sal ammoniac was employed instead of common salt, there were obtained the double chlorides of ammonium and silver, ammonium and lead, &c.

Double iodides.

The double iodides were formed in the same way. M. Becquerel put into one branch of the tube a solution of sulphate of copper, into the other, of hydriodate of potash. Copper in the metallic state was precipitated, as before, on the end of the lead plate plunged into the copper solution; while, upon the other extremity of the lead plate, long silky filaments appeared, consisting of the double iodide of potassium and lead.

He took a tube shut at one of its extremities, put into the bottom of it any oxide—the black oxide of copper, for example—then he poured over it a solution of nitrate of copper, and plunged into it a plate of copper, descending to the very bottom of the tube. In a fortnight there were deposited upon the plate of copper, above the oxide, red octahedral crystals of suboxide of copper. With the subacetate of lead, a plate of lead and litharge, we obtain the protoxide of lead in pentagonal dodecahedrons, or in silky filaments.

Sulphurets, &c.

In 1829, M. Becquerel prosecuted these experimental investigations, and succeeded in forming sulphurets, iodides, and bromides, by similar processes.* He had conceived the idea, that certain compounds had been formed by means of succes-

* Ann. de Chim. et de Phys. xlii. 225.

Part II. sive decompositions; and this idea was verified during these investigations. To obtain sulphuret of silver, such as it occurs in nature, he employed a glass tube, curved in the form of a U, having a quantity of moist clay at the curvation, in order to keep the two liquids employed distinct. Into one of the legs of the tube he poured a solution of hyposulphite of potash, and into the other a solution of nitrate of silver; and then established a communication between the upper portion of the two liquids, by means of a silver wire. A very weak electric circuit was formed; the extremity of the silver wire, plunged into the nitrate, became the negative pole, and the other extremity the positive pole. The nitrate of silver was decomposed, and the silver was precipitated in the metallic state. The oxygen and nitric acid being transferred to the other pole, determined the formation of a double hyposulphite, the first by oxydizing the silver, and the second by decomposing the hyposulphite; the consequence was the formation of a double hyposulphite in fine crystals. The nitric acid still continuing to come to that pole, acted upon the double hyposulphite, decomposed it, and occasioned the formation of very regular crystals of sulphuret of silver: hence the crystallization of the sulphuret is owing to the slow decomposition of the double hyposulphite.

The energy of this apparatus being very feeble, the silver deposited at the negative pole has time to assume a regular crystalline form. By proceeding in the same manner, crystals of all the other metals may be obtained.

If for silver and its nitrate, we substitute copper and its nitrate, we form a double hyposulphite of copper and potash, which crystallizes in very fine silky needles. By continuing the action, this double salt is decomposed, and crystals of sulphuret of copper with triangular faces are obtained. By a similar process the oxysulphuret of antimony may be formed. He obtained, likewise, sulphurets of tin, lead, mercury, iron and zinc.

Iodides. M. Becquerel followed the same process in order to obtain the metallic iodides, the crystals were very distinct. He substituted hydriodate of potash or soda for the alkaline hyposul-

phite. With lead he obtained first a double iodide of lead and potassium, which crystallized in very fine white silky needles. By little and little that compound was decomposed, beginning at the under part contiguous to the clay. A great number of octahedral crystals were observed, of a golden yellow colour and a brilliant appearance. These crystals constituted iodide of lead. Copper subjected to the same treatment gave, at first, a double iodide in white needles; this being decomposed in its turn, beautiful crystals of iodide of copper were formed. Ch. XVIII

M. Becquerel carried these successive decompositions and crystallizations much farther, in a paper read to the French Academy, in January, 1830.* It had been shown by Davy, that when various solutions are mixed together and exposed to electric currents, the acids and oxygen pass to the positive pole, while the bases and hydrogen make their way to the negative pole. The same thing happens when the solutions are put into two capsules, communicating with each other by moistened amianthus. This always takes place when the electrical force is great enough to produce decomposition in the solutions; but if it be strong enough to decompose one only, then the elements of this one are transported into the other capsule, where they generally produce modifications which lead to the formation of new compounds.

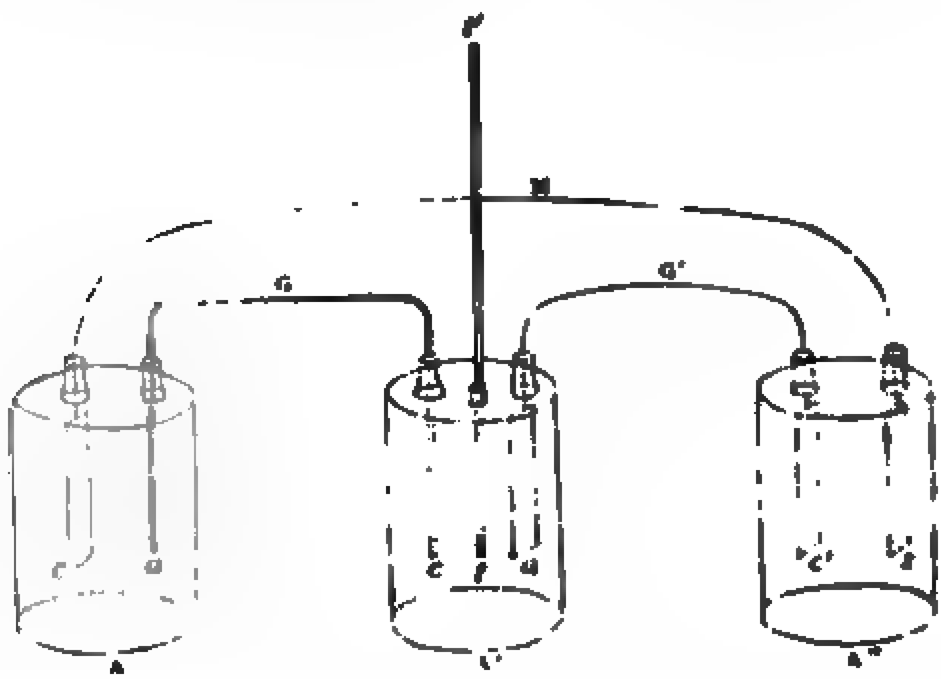
Double effects.

M. Becquerel in this paper examines, in the first place, the case when the metal found at the positive pole of a solution, concurs by the reaction of its oxide to the formation of compounds. He took two glass vessels; into the one he poured a solution of sulphate of copper, and into the other an alcoholic solution of sulpho-carbonate of potash. The communication was established between these glasses by means of an inverted glass syphon, filled with clay moistened with a solution of nitrate of potash, and by means of an arc composed of two plates of copper and lead—the copper dipping into the sulphate, and the lead into the sulpho-carbonate. An electrical current is produced by this arrangement, which acts in the following manner:—Copper is precipitated in the glass con-

* Ann. de Chim. et de Phys. xliii. 131.

Part II. taining the sulphate. Bicarbonate of potash is deposited in crystals on the sides of the vessel containing the sulpho-carbonate. A double sulpho-carbonate of lead and potash is deposited in the same vessel, in acicular crystals, together with carbonate of lead, and probably sulphate of potash, and sulphate of lead. Finally, octahedral crystals of sulphur with rhomboidal bases, were also deposited. In this experiment, the property which sulphuric acid possesses of separating the weaker acids from the bases with which they were united, exerts its force only in consequence of the feebleness of the electric current. Were its energy greater, all the acids, without distinction, would be transferred to the positive pole.

M. Becquerel gives a description of an apparatus which allows the experimenter to avoid or employ at pleasure the reaction of metallic oxides. It may be worth while to give the reader an idea of this apparatus:—It consists of three glass



cylindrical vessels, A A′ A″, placed near each other upon the same horizontal plane. The first is filled with a solution of sulphate or nitrate of copper; the second, with a solution of the substance on the constituents of which we wish to operate: and the third, with water rendered slightly conducting by the addition of an acid, or of common salt. A communicates with A′ by means of a bent tube, *a* G *c*, filled with clay mois-

tened with a saline solution, the nature of which depends upon the effect which we wish to produce in A′. A′ and A″ communicate by means of a plate of platinum or gold, a' G′ c'. And finally, A and A″ are united by two metals composed of two plates, Mc and Mz, of copper and zinc. Finally, a tube of safety, t t', is placed in the glass vessel A′, to indicate the pressure produced by the evolution of gas. From this disposition, the extremity a' of the plate of platinum is the positive pole of a small electric pile, the action of which is slow, but constant. When the liquid contained in A′ is a good conductor, the intensity of the current is sufficient to decompose the sulphate of copper in A. When the oxygen passes to a', as well as the sulphuric acid, which in passing into the tube a G c, disengages sometimes the acids which have less affinity than it has for the bases with which they are combined, all the elements pass into the liquid A′, where their slow action on each other occasion different charges.

Sometimes it is necessary to place a fourth glass cylinder between A and A′. Into it a sufficient quantity of the saline solution to be decomposed by the sulphuric acid is put, so that the effects produced in the liquid A′ may not be interrupted when all the salt in the moist clay is decomposed. In this case, when we wish to transfer an electro-negative gas, or a nascent acid, into the liquid in A′, it will be sufficient to place in the clay a solution, which by its reaction on the sulphuric acid coming from the decomposition of the sulphate of copper, allows that gas, or that acid, to be disengaged. But if the object be to transport hydrogen, or an electro-positive gas, we must reverse the means of communication, and put a' G′ c' in place of the tube a G c. Finally, if we replace the platinum plate by a plate of an oxydisable metal, we introduce into the solution the reaction of an oxide, which being in a nascent state, concurs in the formation of the products.

An example or two will serve to render the use of this apparatus familiar. Into the glass cylinder A′ was put an alcoholic solution of sulpho-carbonate of potash, into the glass A, a solution of sulphate of copper, and into the clay of the

Part II.

tube a G c, a solution of nitrate of potash. In twenty-four hours the reaction of the oxygen, and of the nitric acid, on the solution of the sulpho-carbonate of potash was sensible: for upon the platinum plate were observed crystals of sulphur and of bicarbonate of potash.

When we substitute for the alcoholic solution of sulpho-carbonate of potash, an aqueous solution of sulpho-carbonate of barytes, analogous reactions become sensible; sulphur is precipitated in small crystals, and sulphate of barytes crystallizes in prismatic needles.

Crystals of carbonate of lime obtained.

In 1831, M. Becquerel discovered a method of obtaining artificial crystals of carbonate of lime.* Cruikshanks and Daniell had observed that when we expose to the air a solution of sugar and lime, in a large open vessel, the surface becomes covered with small crystals, which fall to the bottom of the vessel, and are speedily replaced by others. These were crystals of carbonate of lime; but the mode of their formation was disputed. M. Becquerel took a glass tube bent into the form of a U, at the curvature of which a plug of cotton was placed to separate the two liquids from each other. Into the legs of this syphon, he poured a solution of 1 part of lime, and 16 parts of sugar, in 100 parts of water. Into each leg of the syphon was plunged a plate of platinum, communicating with the poles of a Voltaic battery, charged with a weak solution of common salt, containing one per cent. of sulphuric acid. Finally, all the openings of the tubes were carefully shut in order to exclude the common air. The reactions were such that carbonate of lime, partly in crystals, was precipitated upon the positive plate: hence it is clear, that the formation of the carbonate must have been owing to the decomposition of the sugar; but the subject was afterwards examined by M. Pelouze, who showed that the only use of the sugar, in the experiments of Cruikshanks and Daniell, was to increase the solubility of the lime, and that the carbonic acid of the carbonate came from the atmosphere.

M. Becquerel was induced by these experiments to investi-

* Ann. de Chim. et de Phys. xlvii. 5

gate the action of sugar and mucilage on metallic oxides, by the intervention of alkalies and earths;* and we may notice his results though the subject is not connected with electric currents. If we mix oxide of copper, newly precipitated, with lime and water, and apply heat, the oxide becomes black, being rendered anhydrous. If we add a little common sugar, a portion of the oxide dissolves, and the liquid assumes a blue colour, similar to that of a solution of oxide of copper in ammonia. Honey and sugar of milk have the same property. Potash and soda behave precisely as lime does, excepting only that their dissolving power, is greater. Barytes and strontian possess that property only feebly. Thus, potash, soda, and lime, have the property of dissolving oxide of copper, even without heat, when any sugar exists in the solution; a character which may enable us to recognise the presence of sugar in solutions of organized substances. Gum possesses no such property. Gum has the power of precipitating gum when added to a solution of gum containing an alkali or an earth.

Ch. XVIII

Action of sugar on oxide of copper.

Reduction of acids.

Experience having proved that the affinity of a body for another greatly facilitates electro-chemical decompositions, Becquerel employed this principle to reduce oxide of iron, magnesia, zirconia, and glucina.† He conceived from reflecting on the chemical reactions which take place in living animals, that vitality developes peculiar electric forces, which, though feeble in appearance, produce nevertheless effects which can only be obtained by affinities of a certain energy. One of the methods of giving probability to this opinion, is to prove that we can produce in inorganic compounds, energetic chemical effects, with feeble electric forces. He took two glass tubes, containing each in its bottom a quantity of pure clay, and plunged half way into a vessel filled with water. Into the first he poured a solution of nitrate of copper, and by a plate of platinum connected it with the positive pole of a Voltaic trough of thirty pairs. Into the second tube, connected in the same way with the negative pole, was put a

* Ann. de Chim. et de Phys. xlvii. 13. † Ibid. xlviii. 337.

Part II. solution of common salt. In this case water alone was decomposed, and not nitrate of copper. The reason obviously was that the oxide of copper could not be transported through the pure water, because it could not form in that liquid a soluble compound. If we operate in the inverse manner, that is, if we put the nitrate of copper into the negative tube, decomposition takes place; the copper being deposited upon the negative pole, and the acid transported through the liquid to the positive pole.

If we pour into a negative tube a solution of sulphate of copper, and into the positive tube a solution of nitrate of potash, the nitric acid is disengaged in this last tube; the potash in passing to the negative tube acts on the sulphate of copper, separating a portion of the oxide of copper, which is reduced, and occasions the double sulphate of copper and potash which crystallizes. As long as sulphate of copper remains to be decomposed, and as there is a sufficient quantity of nitrate of potash in the positive tube, the sulphuric acid is not transported into that tube; but as soon as the double sulphate begins to be decomposed, its presence there may be rendered sensible by nitrate of barytes. We may lay it down as a general rule, that when two salts are in solution, the most soluble is always decomposed in preference of the other.

It was supposed at first that pretty strong electric currents were necessary to decompose solutions of manganese, zinc, iron, tin, and arsenic, and to obtain a certain quantity of the reduced metal at the negative pole. The solutions of titanium, nickel, cobalt, uranium, and chromium, offered at first no sensible traces of decomposition; but M. Becquerel soon discovered that the presence of water was an obstacle to the decomposition of these salts, especially when the oxygen of the water has less affinity for the hydrogen than it has for the metal. When this was the case, water alone might be decomposed. On this account it was necessary to employ very concentrated solutions, or sometimes the oxides themselves slightly moistened.

Even these precautions are not sufficient when we wish to reduce refractory oxides. We must then have recourse to

another method. When the hydrogen gas arrives at the negative pole, it must exert there a reducing power so much the more energetic the longer it remains in the state of nascent gas, a state the most favourable possible to chemical action. The less rapid the current of gas disengaged is, the longer will it remain in the nascent state. But this requires a weak electric current: hence a certain degree of electric force will be more successful in reducing these refractory metals, than a stronger one would be.

Guided by these theoretical considerations, Becquerel reduced from these solutions, and with an exceedingly weak force, iron, zirconium, glucinum, and magnesium, in crystals or crystalline plates.

To reduce iron to the metallic state, he employed a battery of five or six pairs, feebly charged, or even of a single pair. He made use of two tubes arranged as formerly described. Into the negative tube he poured a solution of the protosulphate of iron, and into the other a solution of common salt; and the battery was so weakly charged that the evolution of gas was scarcely sensible. The soda and hydrogen passed into the negative tube. The alkali uniting with the sulphuric acid, formed a double sulphate of iron and soda; while the hydrogen reacting on the oxide at the instant of its precipitation, occasioned its reduction. The iron is deposited upon the negative plate, sometimes in very small crystals, sometimes as if it had been in fusion, (especially when the action is very slow,) and sometimes in small round tubercles more or less crystalline. When the plate is covered with crystals symmetrically arranged, it possesses magnetic polarity, owing doubtless to the want of continuity in the particles of iron; for the permanency of the magnetic properties of steel is probably owing to the interposition of the particles of carbon, which prevents the two kinds of magnetism from combining together after having been separated.

Magnesia is reduced with more difficulty than iron. Becquerel was obliged to employ a battery of thirty pairs of plates. He put into the negative tube chloride of magnesium; which having been formed in a silver vessel, contained a small

Part II.

quantity of that metal, which was favourable to the success of the process. There was first formed upon the platinum plate a grey deposite, then tubercles of the same colour, and finally, octahedral crystals of a silver-white colour, possessing all the characters of magnesium.

Cementation explained.

M. Becquerel employed electric currents to explain how iron is converted into steel by the process of *cementation*, and how in many rocks the elements are transferred from within to without, and replaced by others without any loss of solidity being the consequence. Nature must employ forces capable of transporting these elements in two different directions, and to great distances. Now, electric currents supply all the forces necessary to produce such effects.

When iron and charcoal are in contact at the temperature of ignition, if we bring both of these bodies in contact with an electro-multiplier, we shall find that a strong electric current exists, which passes from the iron into the charcoal—the same direction as that of the current produced by the combination of iron with oxygen, or of a base with an acid. By means of this current, the particles of carbon are transferred, at a red heat, into the interior of the iron, and thus the iron becomes steel.

Crystals of oxides obtained.

In 1832, M. Becquerel showed how, by means of electric currents, crystals of metallic oxides might be obtained.* To dissolve the oxides he employed peroxide of potassium, which, in consequence of its high degree of oxidation, cannot be capable of forming with these oxides combinations so stable as those with potash, which is a more energetic base.

When potash is melted in an open silver crucible, it loses a part of its water, and absorbs oxygen from the atmosphere, forming peroxide of potash, which is decomposed by water, oxygen gas being disengaged. If we put into a silver crucible half a gramme of black oxide of copper, with two or three grammes of caustic potash, prepared by means of alcohol, and keep its temperature for a few minutes at incipient ignition, the oxide of copper will be completely dissolved. When the

* Ann. de Chim. et de Phys. li. 101.

solution cools, oxygen gas is disengaged, flocks of black oxide of copper are disengaged, and small crystals of anhydrous black oxide are formed, some of which have sometimes a length of several millimeters.

When the crucible is kept for some time in a red heat, the black oxide of copper is changed into red oxide, which is obtained in small crystals of a brick-red colour.

When protoxide of lead is substituted for black oxide of copper, we obtain that protoxide crystallized in small cubes or square plates, provided the crucible be kept only a very short time at an incipient red heat. If the red heat be continued for some time, the protoxide of lead passes into the peroxide, which crystallizes in hexagonal plates of a flea-brown colour. Phosphate and sulphate of lead treated in the same way are completely decomposed, while crystals of protoxide and peroxide of lead are formed. By the same process the oxide of cobalt may be obtained in square plates, and the oxide of zinc in needles of a dirty yellow colour.

It has been often remarked, that in chemical operations the walls of the vessel have an influence on the reactions. M. Becquerel attempted to ascertain the nature of this influence.* He took a narrow glass tube, open at both extremities, and introduced by one end a quantity of calcined oxide of cobalt reduced with water into a very fine paste. The aperture was then closed, a silver wire introduced into the tube, and the upper portion of it filled with a solution of chloride of chromium. After a fortnight small dendritical metallic crystals began to be perceived on the inferior part, and on the surface of the tube. He obtained the same effect without employing the silver wire. The oxide of cobalt, which is not easily reduced by the action of Voltaic electricity, yields in this instance to a current apparently feeble, but which, however, must possess considerable energy.

The inner surface of the tube must have exercised a great influence, as the reduction took place above. In the slow action of the chloride of chromium on the oxide of cobalt,

* Ann. de Chim. et de Phys. lii. 243.

Part II. electricity was disengaged. The two fluids must have united along the inner surface of the tube, which served to establish the poles of the small battery.

M. Becquerel's next attempt was to obtain sulphurets by other processes.* He experimented chiefly on sulphurets of lead and of antimony. Into a tube of glass, shut at one end, he put a quantity of sulphuret of mercury; over it was poured a solution of chloride of magnesium, and a metallic plate of lead was passed through the solution to the very bottom of the tube. The tube was now sealed hermetically, and its contents left to the slow action of the electric current induced by their mutual action on each other. In about six weeks a thin metallic film began to be perceived on the inner surface of the tube, just above the sulphuret of mercury. This film detached itself, and its inner side was studded with tetrahedral crystals of pure sulphuret of lead. On opening the tube, a gas was disengaged, having the odour belonging to combinations of sulphur with chlorine and hydrogen. Antimony subjected to a similar treatment, also furnished crystals; but their nature was not so accurately determined, though probably they were sulphuret of antimony.

Such are the most important of the experiments made by M. Becquerel to form a great number of combinations and decompositions by means of weak electric currents. At the meeting of the British Association held at Bristol in 1836, Andrew Crosse, Esq., read to the chemical section the result of a set of similar experiments, on which he had been engaged for several years. Without being aware of what had been previously done by M. Becquerel, Mr Crosse had obtained many similar results, and came to similar conclusions. It will be sufficient if we state here the general result of Mr Crosse's investigations.

Experiments of Mr Crosse. In the neighbourhood of Broomfield, in Somersetshire, there is a cavern, the roof of which is partly studded with arragonite and calcareous spar in fine crystals. The water which flows from that roof contains in the quart about ten

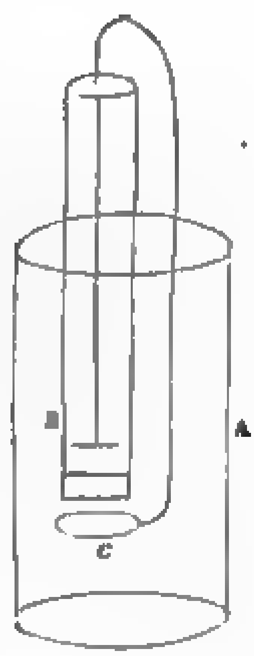
* Ann. de Chim. et de Phys. liii. 105.

substances were upon which these currents acted, and what the specific changes induced.

The experiments of Mr Fox and Mr Henwood constitute an interesting commencement of these very important investigations. The veins of lead ore in the north of England, being so much more numerous, and less deep, than those of Cornwall, and passing through a greater number of beds, would deserve to be experimented on in a similar manner. Is it not probable that the thin layers of iron pyrites so common in pit-coal, owe their origin to electric currents?

Electricity employed to engrave in relief.

Mr Spencer of Liverpool has made a very ingenious application of a Voltaic battery of low intensity, which promises to be exceedingly useful by enabling us to form engravings on copper in relief, which may be substituted for wood-cuts, and thus printed at the same time with the letter-press. The method which he has suggested is this:—Into the glass vessel A, pour a saturated solution of sulphate of copper. Place the glass cylinder B, open at both ends, but having the bottom shut with a plug of plaster of Paris about an inch thick, so that it shall occupy the centre of the vessel A. Fill it with a solution of common salt. C is the copper plate on which the engraving is to be made. A copper wire is to be soldered to it, while a plate of zinc is soldered to the other end of it. The wire is to be so bent that the copper plate C is just under the plaster plug, and of course immersed in the sulphate of copper solution; while the zinc plate is above the plaster plug in the small glass cylinder, and of course immersed in the solution of common salt.



The copper plate C, before being soldered to the wire, is covered with varnish, and the figures or lines wished to be formed upon it in relief are drawn upon the varnish, taking care that every line penetrates to the metal. After the graver has done its duty, pour upon the varnish a little dilute nitric acid, and allow it to remain till the oxide is completely

Part II. removed, and the bottom of every line consists of metalli copper. Now, place the whole as in the figure. The plas ter of Paris plug becoming wetted with the liquid, allows th Voltaic energy to pass, and the circle is complete. Metalli copper is deposited on every line, and adheres firmly to th metallic plate, so that when the varnish is removed, ever line is exhibited in relief. Thus the copper plate may be se up along with the letter-press, precisely like engravings upo wood.

This method will obviously save a good deal of expens and may be applied in a great variety of ways. Mr Spence has very ingeniously pointed out, or alluded to various pu poses to which it may be applied. He has shown how fa similes of coins, medals, &c., may be made, and how engrav ings may be multiplied by it *ad infinitum.* This must b particularly useful in the potteries, where ten or twelve en gravings of the same pattern are often required.

Mr Spencer's pamphlet has been just printed in Liverpool It is entitled, *An Account of some Experiments made for th purpose of ascertaining how far Voltaic Electricity may be us fully applied to the purpose of Working in Metal. Being a paper read at a meeting of the Liverpool Polytechnic Society held at the Medical Institution, Mount Pleasant, on Thursday the 12th September*, 1839. It is a most interesting pamphle of twenty-six pages, and exhibits one of the happiest and mos masterly pieces of inductive reasoning in the English lan guage.

CHAPTER XIX.

OF THE ELECTRICITY OF THE ATMOSPHERE.

After discovering the Leyden phial, and witnessing th effects of electrical batteries, electricians could not avoi perceiving the analogy which existed between lightning an

electricity. This analogy was accordingly pointed out by various individuals, especially by the Abbé Nollet; but it was Dr Franklin, who, having discovered the power of pointed bodies to discharge electricity at a distance, first thought of employing that method to render atmospherical electricity sensible. Not having in America the requisite means for such an experiment, he requested the electricians in Europe to turn their attention to it. The first person who attended to his suggestion was M. D'Abilard, who constructed an apparatus for the purpose at Marly la Ville, about 6 leagues from Paris. It consisted of an iron rod 40 feet long, the lower extremity of which was brought into a sentry-box where rain could not enter; while on the outside it was fastened to three wooden posts by silken strings defended from the rain. M. D'Abilard was obliged to leave home, but in his absence he intrusted his machine to one Coisier, a joiner, who had served 14 years among the dragoons, and on whose courage and understanding he could depend. This artisan had all the requisite instructions given him, and he was desired to call some of his neighbours, particularly the curate of the parish, whenever there should be any appearance of a thunder storm. On Wednesday the 10th of May, 1752, between two and three in the afternoon, Coisier heard a pretty loud clap of thunder. Immediately he ran to the machine, taking with him a phial furnished with a brass wire; and presenting the wire to the end of the rod, a small spark issued from it with a snap like that of a spark from an electrified conductor. Stronger sparks were afterwards drawn in the presence of the curate and a number of other people. The curate's account of them was that they were of a blue colour, an inch and a half in length, and smelled strongly of sulphur. In taking them, Coisier received a stroke on his arm, a little below the elbow.

Lightning and electricity the same.

Verified by D'Abilard.

Eight days after this, M. Delor witnessed the same appearances at his house situated in one of the highest spots in Paris. His apparatus was similar to that of D'Abilard, excepting that his iron rod was 99 feet in length. There was no thunder nor lightning, only a cloud passed over the house.

Dr Franklin, after having published his views on this sub-

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Franklin's electric kite.

ject, was waiting for the erection of a spire in Philadelphia to execute his experiment. It occurred to him that by means of a common kite, he could have a readier and better access to the regions of thunder, than by any spire whatever. Preparing therefore a large silk handkerchief, and two cross sticks of sufficient length on which to extend it, he took the opportunity of the first approaching thunder storm, to take a walk into a field in which there was a shade convenient for his purpose. But dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to nobody but his son, who assisted him in raising the kite.

The kite being raised, a considerable time elapsed before there was any appearance of its being electrified. One very promising cloud had passed over it without effect; when at length, just as he was beginning to despair of his contrivance, the rain having wetted the string which was of hemp, and rendered it a better conductor, he observed some loose threads on it to stand erect, and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key which was tied to the end of the string, and the discovery was complete. He perceived a very evident electric spark. Others succeeded even before the string was fully wet, so as to put the matter past all dispute; and when the rain had wetted the string, he collected sparks very copiously. This happened in June, 1752; about a month after the French electricians had verified the theory, but before any information on the subject had reached Dr Franklin.

Hazard of the experiment.

This experiment of Franklin was attended with extreme danger. Had his cord been more moistened, or a better conductor than it was, it is probable that this celebrated man would have fallen a sacrifice to his temerity, and we would have been deprived of all the benefits which he afterwards conferred upon science, upon his own country, and upon mankind in general. This actually happened soon after to Professor Richmann of Petersburg, who was struck dead by a flash of lightning on the 6th of August, 1753, drawn by his

Death of Professor Richmann.

apparatus into his own room, as he was attending to an experiment which he was making with it.

He had provided himself with an instrument, which he called an electrical gnomon, the use of which was to measure the strength of electricity. It consisted of a rod of metal terminating in a small glass vessel, into which he had (for what reason does not appear,) put some brass filings. At the end of this rod a thread was fastened, which hung down by the side of the rod when it was not electrified, but when it was it avoided the rod, and stood at a distance from it, making an angle at the place where it was fastened. To measure this angle he had the arch of a quadrant fastened to the bottom of the iron rod.

He was observing the effect of the electricity of the clouds, at the approach of a thunder storm upon his gnomon, and of course standing with his head inclined towards it, accompanied by M. Solokow (an engraver, whom he frequently took with him to be a joint observer of his electrical experiments, in order to represent them the better in his figures), when this gentleman, who was standing close to his elbow, observed a globe of blue fire, as he called it, as big as his fist, jump from the rod of the gnomon, towards the head of the Professor, which, at that instant, was about a foot distant from the rod. This flash killed Mr Richmann; but Mr Solokow could give no account of the particular manner in which he was immediately affected by it. For at the same time that the Professor was struck, there arose a sort of steam or vapour, which entirely benumbed him, and made him sink down upon the ground; so that he could not remember even to have heard the clap of thunder, which was very loud.

The globe of fire was attended with a report as loud as that of a pistol. A wire which brought the electricity to the metal rod was broken to pieces, and its fragments thrown upon Mr Solokow's clothes. Half of the glass vessel in which the rod of the gnomon stood, was broken off, and the filings of metal that were in it were thrown about the room. The door-case of the room was half split through, the door torn off and thrown into the room. The shoe on the Professor's

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left foot was burst open, and there was a blue mark on the foot at that place. From which it was concluded, that the electricity had entered by the head, where there were evident marks of injury, and made its way out again by the left foot.*

Improvements in the kite.

M. de Romas made the experiment with the kite in a more perfect manner than the first attempt of Dr Franklin. He twisted a fine iron wire into the cord of the kite.† To prevent the observer from being exposed to danger, the lower extremity of the string terminated in a silk cord eight or ten feet in length, by means of which the kite with its string was insulated. Instead of drawing sparks with the finger, which makes the observer himself receive the charge, he received them by means of a metallic conductor, connected with the ground by a chain, which he held in his hand by means of an insulating glass handle, so that it resembled our common discharger. Romas describes the sparks given out from the string to this discharger during a thunder storm, in a letter to the Abbé Nollet, in very glowing language. "Conceive," says he, "plates of flame nine or ten feet long, and an inch thick, which made as much noise as a pistol. In less than an hour I had certainly thirty plates of this size, without reckoning a thousand others of seven feet, and below that. But what gave me the greatest satisfaction in this new spectacle, was, that the greatest of these plates were spontaneous, and that notwithstanding the abundance of the fire which they contained, they fell always on the nearest conductor. This

* See Phil. Trans. 1754, vol. xlviii. p. 765. Vol. xlix. p. 61. Professor Richmann was born in Livonia, on the 11th July, 1711, his father, William Richmann, having died of the plague before the birth of his son. He was educated at Revel, Halle, and Jena, and devoted himself to mathematics and physics. He went to Petersburg as tutor to the children of Count Ostermann. In 1735, he became an adjunct of the Academy. In 1741, he was made extraordinary Professor of Physics, and in 1745, on the death of Krafft, he became ordinary Professor of Physics. Though his papers are valuable, his death has given him a greater degree of celebrity than he would otherwise probably have attained. See Novi Comment. Acad. Petropol. tom. iv. for 1752–3, p. 36.

† Mr Cuthbertson employed a brass wire which is better, and M Charles used a cord all of metal threads

constancy gave me so much security, that I was not afraid to draw sparks by means of my conductor, even when the thunder storm was at its height, although the glass handle of that instrument was only two feet in length. I conducted where I pleased without feeling in my hand the smallest commotion, sparks of firé six or seven feet long, with the same facility as those whose length did not exceed seven or eight inches."

I refer those readers, who may wish to employ an electrical kite for similar purposes, to the late Mr Cuthbertson's *Practical Electricity*, p. 237, where they will find the contrivances which he employed to secure the observer against all risk of danger. It may, however, be proper to observe, that the experimenter should never hold the string of the kite in his hand, even though the lower part of it be of silk, in case rain or any accidental cause should make it a conductor. The tremendous size of the sparks described above by Romas, shows that in such a case they might produce fatal effects.

Intensity of electricity at the top of a rod.

Indeed, it is easy to see how intense the electricity may become at the end of the cord, even when the thunder clouds are not very formidable in appearance. Poisson has demonstrated that in the case of an insulated cylinder of small dimensions in the neighbourhood of a charged conductor, the thickness of the coating of electricity, on the cylinder is inversely, as its diameter compared to the osculating sphere of the charged conductor. Let $r =$ diameter of the cord of the kite, $\mathrm{R} =$ diameter of the osculating sphere of the thunder cloud, $\mathrm{E} =$ thickness of the coating of electricity of the thunder cloud, e that of the string, then it has been demonstrated that $e = m\,\frac{\mathrm{E\,R}}{r}$, m being a constant coefficient, which experiment has shown to be very nearly $\frac{9}{48}$. But the thickness of the coating at the extremity of the cylinder, being to that at any other part, as 2·3 to 1, the expression for it will be $\frac{20{\cdot}7}{48} \cdot \frac{\mathrm{E\,R}}{r}$.

To apply this expression to the thickness of the coating of electricity at the end of the kite string, let us suppose the

Part II. diameter of the string to be one line, while the radius of the osculating circle of the cloud is 1000 feet. Then $R =$ 144,000 lines, and $r = 1$. The expression becomes

$$\frac{20{\cdot}7}{48} \cdot \frac{144{,}000\ E}{1} = 62{,}100\ E.$$

Thus it appears that the thickness of the coating of electricity at the extremity of the cord of the kite, is no less than 62,100 times greater than upon the charged cloud. Now, as the pressure on the air is equal to the square of the thickness, it will be almost 400 million times greater at the end of the string, than on the surface of the cloud. We need not be surprised then that such terrible sparks issue from the end of the string, even when the thunder cloud is not very considerable.

Thus, it has been demonstrated by the sagacity of Dr Franklin, that *thunder and lightning* is merely a case of electrical discharges from one portion of the atmosphere to another, or from one cloud to another. Air, and all gases, are non-conductors; but vapour and clouds, which are composed of it, are conductors. Clouds consist of small hollow bladders of vapour, charged each with the same kind of electricity. It is this electric charge which prevents the vesicles from uniting together, and falling down in the form of rain. Even the vesicular form which the vapour assumes, is probably owing to the particles being charged with electricity. The mutual repulsion of the electric particles may be considered as sufficient (since they are prevented from leaving the vesicle by the action of the surrounding air, and of the surrounding vesicles,) to give the vapour the vesicular form.

Formation of clouds.

In what way these clouds come to be charged with electricity, it is not easy to say. But as electricity is evolved during the act of evaporation,* the probability is, that clouds are

* M. Pouillet has published a set of experiments, which seem to overturn Volta's theory of the evolution of electricity by evaporation. He has shown that no electricity is evolved by evaporation, unless some chemical combination takes place at the same time. But it follows from his experiments that electricity is evolved abundantly during combustion. The burning body giving out resinous, and the oxygen vitreous electricity. In like manner, the carbonic acid emitted by vegetables is charged

always charged with electricity, and that they owe their existence, or at least their form, to that fluid. It is very probable that when two currents of dry air are moving different ways, the friction of the two surfaces may evolve electricity. Should these currents be of different temperatures, a portion of the vapour which they always contain will be deposited; the electricity evolved will be taken up by that vapour, and will cause it to assume the vesicular state constituting a cloud. Thus we can see in general how clouds come to be formed, and how they contain electricity. This electricity may be either vitreous or resinous according to circumstances. And it is conceivable, that by long-continued opposite currents of air, the charge accumulated in a cloud may be considerable. Now, when two clouds charged, the one with positive, and the other with negative, electricity, happen to approach within a certain distance, the thickness of the coating of electricity increases on the two sides of the clouds which are nearest each other. This accumulation of thickness soon becomes so great as to overcome the pressure of the atmosphere, and a discharge takes place, which occasions the flash of lightning. The noise accompanying the discharge constitutes the thunder clap, the long continuance of which partly depends upon the reverberations from neighbouring objects. It is therefore loudest and largest, and most tremendous, in hilly countries.

Description of a thunder storm.

A thunder storm in this country commonly commences in the following manner:—A low dense cloud begins to form in a part of the atmosphere that was previously clear. This cloud increases fast, but only from its upper part, and spreads into an arched form, appearing like a large heap of cotton wool. Its under surface is level, as if it rested on a smooth plane. The wind is hushed, and every thing appears preternaturally calm and still.

Numberless small ragged clouds, like teazled flakes of

with resinous electricity, and the oxygen (probably) charged with vitreous electricity. These two sources are sufficiently abundant to account for the vast quantity of electricity so often accumulated in the clouds. See Ann. de Chim. et de Phys. xxxv. 401.

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cotton, soon begin to make their appearance, moving about in various directions, and perpetually changing their irregular surface, appearing to increase by gradual accumulation. As they move about they approach each other, and appear to stretch out their ragged arms towards each other. They do not often come in contact; but after approaching very near each other, they evidently recede either in whole, or by bending away their ragged arms.

During this confused motion, the whole mass of small clouds approaches the great one above it; and when near it, the clouds of the lower mass frequently coalesce with each other, before they coalesce with the upper cloud. But as frequently the upper cloud coalesces without them. Its lower surface, from being level and smooth, now becomes ragged, and its tatters stretch down towards the others, and long arms are extended towards the ground. The heavens now darken apace, the whole mass sinks down; wind rises and frequently shifts in squalls; small clouds move swiftly in various directions; lightning darts from cloud to cloud. A spark is sometimes seen coexistent through a vast horizontal extent, of a zigzag shape, and of different brilliancy in different parts. Lightning strikes between the clouds and the earth—frequently in two places at once. A very heavy rain falls—the cloud is dissipated, or it rises high and becomes light and thin.*

These electrical discharges obviously dissipate the electricity, the cloud condenses into water, and occasions the sudden and heavy rain which always terminates a thunder storm. The previous motions of the clouds, which act like electrometers, indicate the electrical state of different parts of the atmosphere.

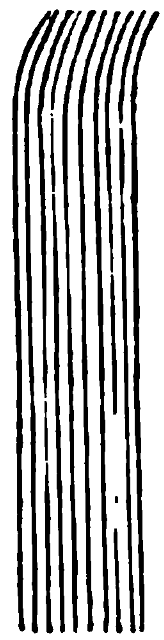
Explanation of the phenomena.

Thunder then only takes place when the different strata of air are in different electrical states. The clouds interposed between these strata, are also electrical, and owe their vesicular nature to that electricity. They are also conductors. Hence they interpose themselves between strata in different

* Robison; Supplement to third Edition of the Encyclopædia Britannica, vol. ii. p. 681.

states, and arrange themselves in such a manner as to occasion the mutual discharge of the strata in opposite states. The equilibrium is restored, the clouds deprived of their electricity collapse into rain, and the thunder terminates.

In thunder storms, the discharges usually take place between two strata of air, very seldom between the air and the earth. But that they are sometimes also between clouds and the earth, cannot be doubted. I once witnessed a thunder storm from the summit of Lochnagar, a very high granite mountain in Aberdeenshire. The thunder was at a great distance east. The first clap was nearly due east, distant not less than ten or twelve miles. The second was north-east, and equally far off. Thick black clouds intervened between me and the thunder, so that I saw no lightning. After watching the progress of the storm for about half an hour, suddenly a white cloud of a very peculiar appearance stretched itself between the part of the heavens where the thunder was, and the earth. This cloud was composed of distinct parallel fibres bent as in the margin: it continued about half an hour, during which, it conveyed away all the surplus electricity from the clouds to the earth. For no more thunder was heard, and the clouds discharged themselves in a heavy shower of rain, which terminated the thunder storm.



Electrical discharges without noise.

These electrical discharges sometimes take place without any noise. In that case the flashes are very bright, but they are single flashes passing visibly from one cloud to another, and confined usually to a single quarter of the heavens. When they are accompanied by the noise, which we call *thunder*, a number of simultaneous flashes, of different colours, and constituting an interrupted zigzag line, may generally be observed stretching to an extent of several miles. These seem to be occasioned by a number of successive or almost simultaneous discharges from one cloud to another; these intermediate clouds serving as intermediate conductors, or stepping-stones for the electrical fluid. It is these simultaneous discharges which occasions the rattling noise, which we call thunder.

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Though they are all made at the same time, yet as their distances are different, they only reach our ear in succession, and thus occasion the lengthened rumbling noise so different from the snap, which accompanies the discharge of a Leyden jar.

Nature of the thunder clap.

If the electricity were confined to the clouds, a single discharge (or a single flash of lightning) would restore the equilibrium. The cloud would collapse and discharge itself in rain, and the serenity of the heavens would be restored. But this is seldom the case. I have witnessed the most vivid discharges of lightning from one cloud to another, which enlightened the whole horizon, continue for several hours, and amounting to a very considerable number, not fewer certainly than fifty, and terminating at last in a violent thunder storm. We see that these discharges, though the quantity of electricity must have been immense, did not restore the equilibrium. It is obvious from this, that not only the clouds but the strata of air themselves, must have been strongly charged with electricity. The clouds being conductors served the purpose of discharging the electricity with which they were loaded, when they came within the striking distance. But the electric stratum of air with which the cloud was in contact, being a non-conductor, would not lose its electricity by the discharge of the cloud. It would immediately supply the cloud with which it was in contact with a new charge. And this repeated charging and discharging process would continue to go on till the different strata of excited air were brought to their natural state.

Electrical state of the atmosphere

From the atmospherical electric journal kept by Mr Read at Knightsbridge, during two whole years from the 9th of May, 1789, to the 9th of May, 1791, it appears that clouds, and rain, and hail, and snow, are always charged with electricity: sometimes with negative, but more frequently with positive electricity. When the sky is serene and cloudless, the strata of air are generally charged with positive electricity. In such cases the thunder rod is charged by induction; the highest end acquiring the opposite state of electricity from the air, and the lowest end the same kind of electricity, while a portion of the rod towards the middle is neutral. During the

first year, there occurred only seven days in which no electricity could be perceived. But during the second year, when the apparatus was much more complete, not a single day occurred which did not give indications of electricity in the atmosphere.

During the first year, the electricity was positive 241 times, and during the second, 423 times. This difference was chiefly owing to the apparatus. During the first year, there occurred seventy-three days in which the signs of electricity were so weak, that the kind could not be determined; and there were seven days in which no signs of electricity at all were perceptible. But during the second year, the apparatus was so much more perfect, that no day occurred without signs of electricity; and it was ascertained, that on those days in which the electricity is weak, it is always positive. During the first year, the electricity was observed negative 156 times, and during the second year, 157 times. During the first year, sparks could be drawn from the apparatus during ninety-eight days, and in the second year, during 106 days. From these facts, the probability is, that the electrical state of the atmosphere did not differ much in each of the two years, during which the observations were kept.

Observations in Germany.

The observations of Read have been prosecuted in Italy and Germany, where, during the summer season, the air is generally much drier than in Great Britain, and therefore, better fitted for electrical observations. The general result is as follows :—

During fine weather, the atmosphere always contains an excess of positive electricity. This excess is very small a little before sunrise. It augments as the sun rises, at first very slowly, and afterwards more rapidly, and reaches its maximum some hours after sunrise. It then begins to diminish at first rapidly, then slowly, and reaches its minimum a few hours before sunset. It begins to augment as soon as the sun begins to approach the horizon, and a few hours after, reaches a second maximum; then it diminishes till toward the rising of the sun, when the same alternations are repeated.

Schubler, by his experiments and observations, has given

Part II. greater precision to these general statements.* His expe ments are too numerous, and occupy too much room to perm us to insert them all here. Very copious extracts from the have been copied by Becquerel into the fourth volume of l *Electricity* (p. 85). The following table exhibits the result hourly observations made on the intensity of the electricity the atmosphere, at Stuttgard, on the 12th of May, 1811.

Hour.	Electrometer.	Saussure's Hygrometer.	Thermometer Centigrade.	Weather.
Morning.				
4 o'Clock,	+ 5	88°	9°·3	
5 . .	+ 6½	88	9·5	Quite serene. Th
6 . .	+ 8	87	10·5	horizon became gra
7 . .	+11	86	12·1	ually foggy. Dew fel
8 . .	+13	84	13·5	
9 . .	+10	76	15·6	
10 . .	+ 8	70	17	The horizon be
Noon, .	+ 7	63	20·1	came clear, and th
2 P.M. .	+ 6½	61	21·6	sky assumed a pure
4 . .	+ 5½	60	21·3	blue colour.
5 . .	+ 5	62	20·9	
6 . .	+ 6	65	20·0	Vapours again mad
7½ . .	+ 8	72	17·5	their appearance.
8½ . .	+12	83	15·5	Evening, dew fell.
9½ . .	+ 8	86	13	
10½ . .	+ 7	88	12·1	Quite serene.
Midnight,	+ 6½	88	11	
2 A.M. .	+ 5½	88	10·1	

From these observations we see, that at Stuttgard, on th 12th of May, 1811, the positive electricity of the atmosphe was at a maximum at eight in the morning. It became gr dually feebler and feebler, and was at its minimum at five, P.M From that time it became stronger, and reached a secon maximum at half-past eight, P.M. From this time it sun again, and reached a second minimum about two in the mor ing, when Schubler discontinued his observations. From other observations of Schubler, which we cannot find room f here, it appears that the maxima and minima show occasional some little differences in the time when they occur. The

* Schweigger's Jour. iii. 123, 268.

for example, in the beginning of June, 1811, the maxima took place most frequently about seven, A.M., and nine or half-past nine, P.M.; while, towards the end of that month, they occurred at six, A.M., and ten, P.M. In the first half of July, the electrical periods were not very distinct, owing to the state of the atmosphere. In the last half of that month, the two maxima occurred at seven, A.M., and nine or ten, P.M. According to Schubler, the electric periods were most distinct in those days which were dry and hot, and on which luminous striæ, analogous to the aurora borealis were perceptible in the sky.

During the first ten days of August, the weather was so bad, that observations could not be made. During the last two-thirds of the month, the weather was clearer, the maxima were at eight, A.M., and half-past eight, P.M.

In September, the maxima corresponded usually to half-past eight, A.M., and half-past seven or eight, P.M.

In October the maxima occurred at seven, A.M., and seven, P.M., and the minima at two, P.M., and ten, P.M.

In November, the maxima were at nine, A.M., and half-past six, P.M., and the minima at half past-seven, A.M., and half-past ten, P.M.

Schubler's observations confirmed an opinion that had been long entertained, namely, that the force of electricity for the two maxima and two minima, goes on increasing from the month of July to the month of January, inclusive. So that the greatest intensity happens in winter, and the weakest in summer: hence we find that in serene days of winter the augmentation of the electricity is always proportional to the increase of the cold. From the mean of the months it appears that the first minimum and the first maximum have less intensity than the second minimum and second maximum.

In Paris.

M. Arago has made a careful set of observations on the electricity of the atmosphere, at the Paris observatory. He has confirmed the occurrence of two maxima and two minima, every twenty-four hours.*

* See a set of his observations inserted by Becquerel in his *Traité l'Electricité*, iv. 93.

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electricity was disengaged. The two fluids must have united along the inner surface of the tube, which served to establish the poles of the small battery.

M. Becquerel's next attempt was to obtain sulphurets by other processes.* He experimented chiefly on sulphurets of lead and of antimony. Into a tube of glass, shut at one end, he put a quantity of sulphuret of mercury; over it was poured a solution of chloride of magnesium, and a metallic plate of lead was passed through the solution to the very bottom of the tube. The tube was now sealed hermetically, and its contents left to the slow action of the electric current induced by their mutual action on each other. In about six weeks a thin metallic film began to be perceived on the inner surface of the tube, just above the sulphuret of mercury. This film detached itself, and its inner side was studded with tetrahedral crystals of pure sulphuret of lead. On opening the tube, a gas was disengaged, having the odour belonging to combinations of sulphur with chlorine and hydrogen. Antimony subjected to a similar treatment, also furnished crystals; but their nature was not so accurately determined, though probably they were sulphuret of antimony.

Such are the most important of the experiments made by M. Becquerel to form a great number of combinations and decompositions by means of weak electric currents. At the meeting of the British Association held at Bristol in 1836, Andrew Crosse, Esq., read to the chemical section the result of a set of similar experiments, on which he had been engaged for several years. Without being aware of what had been previously done by M. Becquerel, Mr Crosse had obtained many similar results, and came to similar conclusions. It will be sufficient if we state here the general result of Mr Crosse's investigations.

Experiments of Mr Crosse.

In the neighbourhood of Broomfield, in Somersetshire, there is a cavern, the roof of which is partly studded with arragonite and calcareous spar in fine crystals. The water which flows from that roof contains in the quart about ten

* Ann. de Chim. et de Phys. liii. 105.

pieces of iron wire crossing at right angles, and attached to the axis of the horse-shoe magnet, and moving round when that axis is set in motion. At every revolution of the axis, the extremities of the iron wires dip four times into the mercury, so that the contact is renewed and broken four times successively at each revolution. The axis of the horse-shoe magnet is fixed horizontally, and connected by a strap to a wheel which can be turned rapidly by a handle, causing the horse-shoe magnet to revolve rapidly about its axis.

The pieces of soft iron surrounded by the helices of copper wire being fixed, when the magnet revolves, its two poles come alternately in contact with each piece during each revolution. It is well known that a piece of soft iron, when in the neighbourhood of a magnetic pole, acquires the same magnetic properties as that pole. But as soon as it leaves the proximity of that pole it loses its magnetism, and acquires the opposite state when it gets into the neighbourhood of the other pole. Thus the magnetic state of each piece of soft iron, and consequently of the surrounding helices, changes with each revolution. This causes a perpetual interruption and renovation of the circuit: hence the sparks, which appear only when a current is interrupted or renewed.

By this apparatus constant sparks may be obtained, fine platinum wire may be ignited, and various chemical phenomena may be produced.

5. When an electric current passes through a wire covered with silk thread, the wire becomes a magnet. If this wire is twisted round, in the form of a helix, every revolution constitutes a distinct magnet having two poles. As these poles are always at the same extremities of the portion of the wire, it is obvious that the force exerted by this helix must increase in proportion to the number of turns which it makes: hence the theory of the Galvano-multiplier of Schweigger, with all its subsequent improvements.

6. It has been shown by the experiments of Biot and Savart, that when an electric current passes near a magnetic needle, the action of the current on the needle is inversely as the distance. They have shown also, that if the elementary force be

Part II. inversely as the square of the distance, then the action o long wire will be inversely as the distance.*

CHAPTER XVIII.

OF DECOMPOSITIONS BY WEAK ELECTRIC CURREN

IT has been long known that certain metals have the prope of precipitating other metals from their solutions in the meta state. Thus, if a plate of copper be put into a solutio nitrate of silver, the copper dissolves while the silver is p cipitated in the metallic state. Mercury equally throws do silver from the nitrate. Lead or tin throws down copper; a zinc or iron is capable of throwing down lead and tin.] Sylvester first showed, by some ingenious experiments, 1806,† that these precipitations were produced by elec currents; that water was decomposed by these currents; a that the hydrogen evolved united with the oxygen of t metallic oxide, and thus caused the metal to separate in t metallic state.

Ritter had already observed, in 1800, that these electri effects are still more sensible when the circuit is made means of two liquids and a metal.‡ In 1807, Bucholz§ ma some experiments on these feeble circuits, which deserve be noticed. He put into a cylindrical glass vessel, a soluti of 1 part of green chloride of copper in 4 parts of water; on this he poured, cautiously, 6 parts of water, so that the wat floated on the surface of the chloride. A polished plate copper, half an inch broad and six inches in length, was ma to pass through the two liquids. In two hours the copp plate was covered with a white matter, which was dichlori of copper. He repeated his experiments, substituting a co

* Becquerel, ii. 432. † Nicholson's Jour. xiv. 95.
‡ Histoire de Galvanism par Sue aîné, ii. 116.
§ Gehlen's Journal, (second series,) v. 127.

centrated solution of nitrate of copper, for the chloride. A portion of the copper plate at the line of separation of the two liquids remained brilliant and untarnished. The portion of copper above this line was covered with black oxide; the under portion of the plate was covered with small grains of metallic copper. The result was the same when the water was acidulated with nitric acid.

When nitrate of silver was substituted for nitrate of copper, and a plate of silver for that of copper, the result was the same. Metallic silver in minute cubes was deposited upon the under portion of the silver plate, and oxide of silver upon the upper portion. Similar experiments were made with nitrate of lead and a plate of lead, chloride of zinc and a plate of zinc, sulphate of iron and a plate of iron, with similar results.

Precipitation of metals on metallic plates.

In 1826 the subject was taken up by M. Becquerel,* to whom we are indebted for a very complete investigation of this interesting subject, and for applying it to explain the numerous changes which are continually occurring in the metallic veins, and even the rocks constituting the surface of the earth. Into a glass tube, bent in the form of a U, he poured a solution of sulphate or nitrate of copper; a copper wire was plunged into each leg of the tube, and these wires were brought into connexion with a small thermo-electric apparatus. The copper wire connected with the negative pole became covered with metallic copper, while the other extremity was oxydized. Solutions of tin, zinc, silver, and lead, with wires of the same metals gave similar results. Wires of platinum and gold in the same solutions produced no effect; the case was different when platinum or gold wire was made to act on nitrate of silver.

From these facts he concluded, that by very feeble electric forces, metals easily reducible are precipitated from their solutions, by plates of the same metal as that held in solution. Becquerel, in these first experiments, showed that decompositions of metallic solutions are produced most easily by the action of metals the same with those held in solution. He

* Ann. de Chim. et de Phys. xxxiv. 152.

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gave a description of a small apparatus, capable of continuing active for months, by means of which these decompositions may be easily induced.

It consists of two glasses, the one containing nitric acid and the other a solution of potash. Into each of them is plunged a plate of platinum, to which a wire is united; and the communication between the two liquids is established by means of a bent glass tube filled with clay, moistened with an aqueous solution of common salt. The reaction of the acid and alkaline solutions on that of the common salt, evolves enough of electricity to produce decompositions. M. Becquerel, in analysing these phenomena, discovered that oxygen is much more easily transferable by electric currents than sulphuric acid.

Formation of chlorides and iodides.

M. Becquerel prosecuted these interesting investigations with assiduity, and in 1827, his experiments on the formation of chlorides and iodides by weak electric currents, were read to the Academy of Sciences of Paris, and published during the same year.* He found that when a neutral salt is substituted for a metallic salt, these feeble electric currents cannot traverse the liquid when we employ wires of gold or platinum to complete the current. With a wire of silver the current is very feeble; but with wires of zinc, lead, iron, and tin, the effects are well marked. This difference depends upon the easy alteration of the oxidable metals, an alteration which facilitates the passage of the electricity of a wire into a solution.

Of double chlorides.

It had been previously ascertained that chlorides have a tendency to combine with each other so as to constitute double chlorides.† Becquerel attempted to form these double chlorides by means of a weak electrical current. The difficulty consists in employing the requisite electrical force. If the intensity is too great, we isolate all the elements; if it is too feeble we remove only one, two, or three of these elements.

* Ann. de Chim. et de Phys. xxxv. 126.

† Some chlorides, as has been shown by Mr Bonsdorf, have the characters of an acid, and others those of a base: hence they combine and form compounds analogous to the salts formed by the union of oxygen acids with oxygen bases.

He put a plug of amianthus into the bottom of a glass tube bent into the shape of a U. Into one of the branches he put a solution of sulphate of copper, and into the other a solution of common salt; then the communication between the two liquids was completed by a plate of copper. In a short time that end of the plate which was plunged into the sulphate of copper, became covered with metallic copper; on the other extremity of the plate were deposited octahedral crystals of the double chloride of sodium and copper, which are decomposed by the contact of water. Similar double chlorides were obtained when solutions of silver and of other metals were substituted for the sulphate of copper. When sal ammoniac was employed instead of common salt, there were obtained the double chlorides of ammonium and silver, ammonium and lead, &c.

Double iodides.

The double iodides were formed in the same way. M. Becquerel put into one branch of the tube a solution of sulphate of copper, into the other, of hydriodate of potash. Copper in the metallic state was precipitated, as before, on the end of the lead plate plunged into the copper solution; while, upon the other extremity of the lead plate, long silky filaments appeared, consisting of the double iodide of potassium and lead.

He took a tube shut at one of its extremities, put into the bottom of it any oxide—the black oxide of copper, for example—then he poured over it a solution of nitrate of copper, and plunged into it a plate of copper, descending to the very bottom of the tube. In a fortnight there were deposited upon the plate of copper, above the oxide, red octahedral crystals of suboxide of copper. With the subacetate of lead, a plate of lead and litharge, we obtain the protoxide of lead in pentagonal dodecahedrons, or in silky filaments.

Sulphurets, &c.

In 1829, M. Becquerel prosecuted these experimental investigations, and succeeded in forming sulphurets, iodides, and bromides, by similar processes.* He had conceived the idea, that certain compounds had been formed by means of succes-

* Ann. de Chim. et de Phys. xlii. 225.

Part II. sive decompositions; and this idea was verified during these investigations. To obtain sulphuret of silver, such as it occurs in nature, he employed a glass tube, curved in the form of a U, having a quantity of moist clay at the curvation, in order to keep the two liquids employed distinct. Into one of the legs of the tube he poured a solution of hyposulphite of potash, and into the other a solution of nitrate of silver; and then established a communication between the upper portion of the two liquids, by means of a silver wire. A very weak electric circuit was formed; the extremity of the silver wire, plunged into the nitrate, became the negative pole, and the other extremity the positive pole. The nitrate of silver was decomposed, and the silver was precipitated in the metallic state. The oxygen and nitric acid being transferred to the other pole, determined the formation of a double hyposulphite, the first by oxydizing the silver, and the second by decomposing the hyposulphite; the consequence was the formation of a double hyposulphite in fine crystals. The nitric acid still continuing to come to that pole, acted upon the double hyposulphite, decomposed it, and occasioned the formation of very regular crystals of sulphuret of silver: hence the crystallization of the sulphuret is owing to the slow decomposition of the double hyposulphite.

The energy of this apparatus being very feeble, the silver deposited at the negative pole has time to assume a regular crystalline form. By proceeding in the same manner, crystals of all the other metals may be obtained.

If for silver and its nitrate, we substitute copper and its nitrate, we form a double hyposulphite of copper and potash, which crystallizes in very fine silky needles. By continuing the action, this double salt is decomposed, and crystals of sulphuret of copper with triangular faces are obtained. By a similar process the oxysulphuret of antimony may be formed. He obtained, likewise, sulphurets of tin, lead, mercury, iron, and zinc.

Iodides. M. Becquerel followed the same process in order to obtain the metallic iodides, the crystals were very distinct. He substituted hydriodate of potash or soda for the alkaline hyposul-

phite. With lead he obtained first a double iodide of lead and potassium, which crystallized in very fine white silky needles. By little and little that compound was decomposed, beginning at the under part contiguous to the clay. A great number of octahedral crystals were observed, of a golden yellow colour and a brilliant appearance. These crystals constituted iodide of lead. Copper subjected to the same treatment gave, at first, a double iodide in white needles; this being decomposed in its turn, beautiful crystals of iodide of copper were formed. Ch. XVIII.

M. Becquerel carried these successive decompositions and crystallizations much farther, in a paper read to the French Academy, in January, 1830.* It had been shown by Davy, that when various solutions are mixed together and exposed to electric currents, the acids and oxygen pass to the positive pole, while the bases and hydrogen make their way to the negative pole. The same thing happens when the solutions are put into two capsules, communicating with each other by moistened amianthus. This always takes place when the electrical force is great enough to produce decomposition in the solutions; but if it be strong enough to decompose one only, then the elements of this one are transported into the other capsule, where they generally produce modifications which lead to the formation of new compounds.

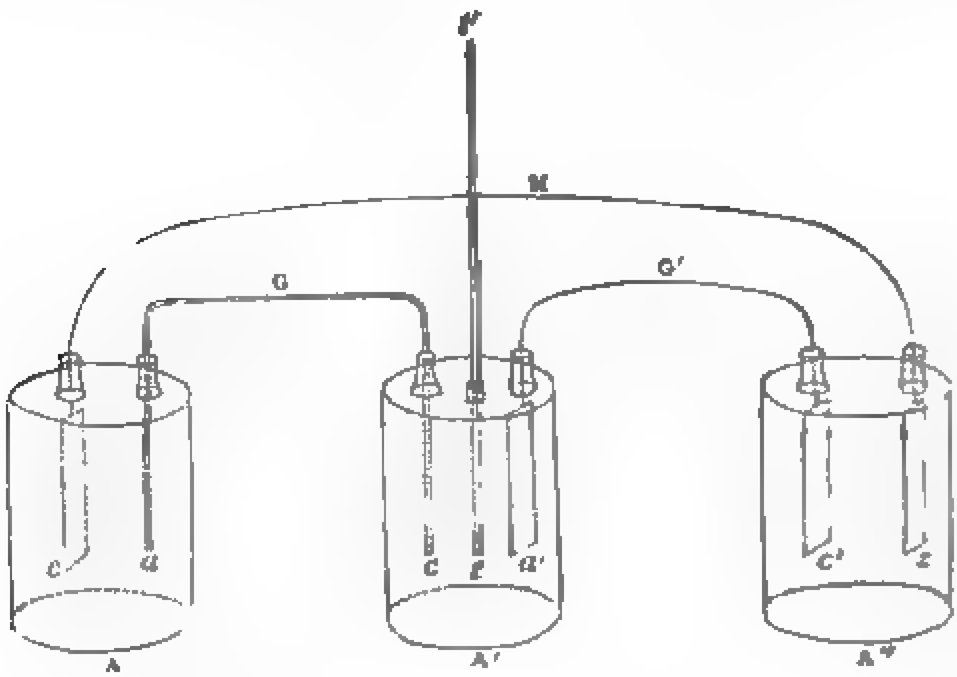
Double effects.

M. Becquerel in this paper examines, in the first place, the case when the metal found at the positive pole of a solution, concurs by the reaction of its oxide to the formation of compounds. He took two glass vessels; into the one he poured a solution of sulphate of copper, and into the other an alcoholic solution of sulpho-carbonate of potash. The communication was established between these glasses by means of an inverted glass syphon, filled with clay moistened with a solution of nitrate of potash, and by means of an arc composed of two plates of copper and lead—the copper dipping into the sulphate, and the lead into the sulpho-carbonate. An electrical current is produced by this arrangement, which acts in the following manner:—Copper is precipitated in the glass con-

* Ann. de Chim. et de Phys. xlii. 131.

Part II. taining the sulphate. Bicarbonate of potash is deposited in crystals on the sides of the vessel containing the sulpho-carbonate. A double sulpho-carbonate of lead and potash is deposited in the same vessel, in acicular crystals, together with carbonate of lead, and probably sulphate of potash, and sulphate of lead. Finally, octahedral crystals of sulphur with rhomboidal bases, were also deposited. In this experiment, the property which sulphuric acid possesses of separating the weaker acids from the bases with which they were united, exerts its force only in consequence of the feebleness of the electric current. Were its energy greater, all the acids, without distinction, would be transferred to the positive pole.

M. Becquerel gives a description of an apparatus which allows the experimenter to avoid or employ at pleasure the reaction of metallic oxides. It may be worth while to give the reader an idea of this apparatus:—It consists of three glass



cylindrical vessels, A A′ A″, placed near each other upon the same horizontal plane. The first is filled with a solution of sulphate or nitrate of copper; the second, with a solution of the substance on the constituents of which we wish to operate; and the third, with water rendered slightly conducting by the addition of an acid, or of common salt. A communicates with A′ by means of a bent tube, *a* G *c*, filled with clay mois-

tened with a saline solution, the nature of which depends upon the effect which we wish to produce in A′. A′ and A″ communicate by means of a plate of platinum or gold, *a′* G′ *c′*. And finally, A and A″ are united by two metals composed of two plates, M*c* and M*z*, of copper and zinc. Finally, a tube of safety, *t t′*, is placed in the glass vessel A′, to indicate the pressure produced by the evolution of gas. From this disposition, the extremity *a′* of the plate of platinum is the positive pole of a small electric pile, the action of which is slow, but constant. When the liquid contained in A′ is a good conductor, the intensity of the current is sufficient to decompose the sulphate of copper in A. When the oxygen passes to *a′*, as well as the sulphuric acid, which in passing into the tube *a* G *c*, disengages sometimes the acids which have less affinity than it has for the bases with which they are combined, all the elements pass into the liquid A′, where their slow action on each other occasion different charges.

Sometimes it is necessary to place a fourth glass cylinder between A and A′. Into it a sufficient quantity of the saline solution to be decomposed by the sulphuric acid is put, so that the effects produced in the liquid A′ may not be interrupted when all the salt in the moist clay is decomposed. In this case, when we wish to transfer an electro-negative gas, or a nascent acid, into the liquid in A′, it will be sufficient to place in the clay a solution, which by its reaction on the sulphuric acid coming from the decomposition of the sulphate of copper, allows that gas, or that acid, to be disengaged. But if the object be to transport hydrogen, or an electro-positive gas, we must reverse the means of communication, and put *a′* G′ *c′* in place of the tube *a* G *c*. Finally, if we replace the platinum plate by a plate of an oxydisable metal, we introduce into the solution the reaction of an oxide, which being in a nascent state, concurs in the formation of the products.

An example or two will serve to render the use of this apparatus familiar. Into the glass cylinder A was put an alcoholic solution of sulpho-carbonate of potash, into the glass A, a solution of sulphate of copper, and into the clay of the

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tube a G c, a solution of nitrate of potash. In twenty-four hours the reaction of the oxygen, and of the nitric acid, on the solution of the sulpho-carbonate of potash was sensible; for upon the platinum plate were observed crystals of sulphur and of bicarbonate of potash.

When we substitute for the alcoholic solution of sulpho-carbonate of potash, an aqueous solution of sulpho-carbonate of barytes, analogous reactions become sensible; sulphur is precipitated in small crystals, and sulphate of barytes crystallizes in prismatic needles.

Crystals of carbonate of lime obtained.

In 1831, M. Becquerel discovered a method of obtaining artificial crystals of carbonate of lime.* Cruikshanks and Daniell had observed that when we expose to the air a solution of sugar and lime, in a large open vessel, the surface becomes covered with small crystals, which fall to the bottom of the vessel, and are speedily replaced by others. These were crystals of carbonate of lime; but the mode of their formation was disputed. M. Becquerel took a glass tube bent into the form of a U, at the curvature of which a plug of cotton was placed to separate the two liquids from each other. Into the legs of this syphon, he poured a solution of 1 part of lime, and 16 parts of sugar, in 100 parts of water. Into each leg of the syphon was plunged a plate of platinum, communicating with the poles of a Voltaic battery, charged with a weak solution of common salt, containing one per cent. of sulphuric acid. Finally, all the openings of the tubes were carefully shut in order to exclude the common air. The reactions were such that carbonate of lime, partly in crystals, was precipitated upon the positive plate: hence it is clear, that the formation of the carbonate must have been owing to the decomposition of the sugar; but the subject was afterwards examined by M. Pelouze, who showed that the only use of the sugar, in the experiments of Cruikshanks and Daniell, was to increase the solubility of the lime, and that the carbonic acid of the carbonate came from the atmosphere.

M. Becquerel was induced by these experiments to investi-

* Ann. de Chim. et de Phys. xlvii. 5.

gate the action of sugar and mucilage on metallic oxides, by the intervention of alkalies and earths;* and we may notice his results though the subject is not connected with electric currents. If we mix oxide of copper, newly precipitated, with lime and water, and apply heat, the oxide becomes black, being rendered anhydrous. If we add a little common sugar, a portion of the oxide dissolves, and the liquid assumes a blue colour, similar to that of a solution of oxide of copper in ammonia. Honey and sugar of milk have the same property. Potash and soda behave precisely as lime does, excepting only that their dissolving power is greater. Barytes and strontian possess that property only feebly. Thus, potash, soda, and lime, have the property of dissolving oxide of copper, even without heat, when any sugar exists in the solution; a character which may enable us to recognise the presence of sugar in solutions of organized substances. Gum possesses no such property. Gum has the power of precipitating gum when added to a solution of gum containing an alkali or an earth.

Action of sugar on oxide of copper.

Experience having proved that the affinity of a body for another greatly facilitates electro-chemical decompositions, Becquerel employed this principle to reduce oxide of iron, magnesia, zirconia, and glucina.† He conceived from reflecting on the chemical reactions which take place in living animals, that vitality developes peculiar electric forces, which, though feeble in appearance, produce nevertheless effects which can only be obtained by affinities of a certain energy. One of the methods of giving probability to this opinion, is to prove that we can produce in inorganic compounds, energetic chemical effects, with feeble electric forces. He took two glass tubes, containing each in its bottom a quantity of pure clay, and plunged half way into a vessel filled with water. Into the first he poured a solution of nitrate of copper, and by a plate of platinum connected it with the positive pole of a Voltaic trough of thirty pairs. Into the second tube, connected in the same way with the negative pole, was put a

Reduction of acids.

* Ann. de Chim. et de Phys. xlvii. 13. † Ibid. xlviii. 337.

 solution of common salt. In this case water alone was decomposed, and not nitrate of copper. The reason obviously was that the oxide of copper could not be transported through the pure water, because it could not form in that liquid a soluble compound. If we operate in the inverse manner, that is, if we put the nitrate of copper into the negative tube, decomposition takes place; the copper being deposited upon the negative pole, and the acid transported through the liquid to the positive pole.

If we pour into a negative tube a solution of sulphate of copper, and into the positive tube a solution of nitrate of potash, the nitric acid is disengaged in this last tube; the potash in passing to the negative tube acts on the sulphate of copper, separating a portion of the oxide of copper, which is reduced, and occasions the double sulphate of copper and potash which crystallizes. As long as sulphate of copper remains to be decomposed, and as there is a sufficient quantity of nitrate of potash in the positive tube, the sulphuric acid is not transported into that tube; but as soon as the double sulphate begins to be decomposed, its presence there may be rendered sensible by nitrate of barytes. We may lay it down as a general rule, that when two salts are in solution, the most soluble is always decomposed in preference of the other.

It was supposed at first that pretty strong electric currents were necessary to decompose solutions of manganese, zinc, iron, tin, and arsenic, and to obtain a certain quantity of the reduced metal at the negative pole. The solutions of titanium, nickel, cobalt, uranium, and chromium, offered at first no sensible traces of decomposition; but M. Becquerel soon discovered that the presence of water was an obstacle to the decomposition of these salts, especially when the oxygen of the water has less affinity for the hydrogen than it has for the metal. When this was the case, water alone might be decomposed. On this account it was necessary to employ very concentrated solutions, or sometimes the oxides themselves slightly moistened.

Even these precautions are not sufficient when we wish to reduce refractory oxides. We must then have recourse to

another method. When the hydrogen gas arrives at the negative pole, it must exert there a reducing power so much the more energetic the longer it remains in the state of nascent gas, a state the most favourable possible to chemical action. The less rapid the current of gas disengaged is, the longer will it remain in the nascent state. But this requires a weak electric current: hence a certain degree of electric force will be more successful in reducing these refractory metals, than a stronger one would be. Ch. XVIII

Guided by these theoretical considerations, Becquerel reduced from these solutions, and with an exceedingly weak force, iron, zirconium, glucinum, and magnesium, in crystals or crystalline plates.

To reduce iron to the metallic state, he employed a battery of five or six pairs, feebly charged, or even of a single pair. He made use of two tubes arranged as formerly described. Into the negative tube he poured a solution of the protosulphate of iron, and into the other a solution of common salt; and the battery was so weakly charged that the evolution of gas was scarcely sensible. The soda and hydrogen passed into the negative tube. The alkali uniting with the sulphuric acid, formed a double sulphate of iron and soda; while the hydrogen reacting on the oxide at the instant of its precipitation, occasioned its reduction. The iron is deposited upon the negative plate, sometimes in very small crystals, sometimes as if it had been in fusion, (especially when the action is very slow,) and sometimes in small round tubercles more or less crystalline. When the plate is covered with crystals symmetrically arranged, it possesses magnetic polarity, owing doubtless to the want of continuity in the particles of iron; for the permanency of the magnetic properties of steel is probably owing to the interposition of the particles of carbon, which prevents the two kinds of magnetism from combining together after having been separated.

Magnesia is reduced with more difficulty than iron. Becquerel was obliged to employ a battery of thirty pairs of plates. He put into the negative tube chloride of magnesium; which having been formed in a silver vessel, contained a small

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quantity of that metal, which was favourable to the success of the process. There was first formed upon the platinum plate a grey deposite, then tubercles of the same colour, and finally, octahedral crystals of a silver-white colour, possessing all the characters of magnesium.

Cementation explained.

M. Becquerel employed electric currents to explain how iron is converted into steel by the process of *cementation*, and how in many rocks the elements are transferred from within to without, and replaced by others without any loss of solidity being the consequence. Nature must employ forces capable of transporting these elements in two different directions, and to great distances. Now, electric currents supply all the forces necessary to produce such effects.

When iron and charcoal are in contact at the temperature of ignition, if we bring both of these bodies in contact with an electro-multiplier, we shall find that a strong electric current exists, which passes from the iron into the charcoal—the same direction as that of the current produced by the combination of iron with oxygen, or of a base with an acid. By means of this current, the particles of carbon are transferred, at a red heat, into the interior of the iron, and thus the iron becomes steel.

Crystals of oxides obtained.

In 1832, M. Becquerel showed how, by means of electric currents, crystals of metallic oxides might be obtained.* To dissolve the oxides he employed peroxide of potassium, which, in consequence of its high degree of oxidation, cannot be capable of forming with these oxides combinations so stable as those with potash, which is a more energetic base.

When potash is melted in an open silver crucible, it loses a part of its water, and absorbs oxygen from the atmosphere, forming peroxide of potash, which is decomposed by water, oxygen gas being disengaged. If we put into a silver crucible half a gramme of black oxide of copper, with two or three grammes of caustic potash, prepared by means of alcohol, and keep its temperature for a few minutes at incipient ignition, the oxide of copper will be completely dissolved. When the

* Ann. de Chim. et de Phys. li. 101.

solution cools, oxygen gas is disengaged, flocks of black oxide of copper are disengaged, and small crystals of anhydrous black oxide are formed, some of which have sometimes a length of several millimeters. Ch.XVIII

When the crucible is kept for some time in a red heat, the black oxide of copper is changed into red oxide, which is obtained in small crystals of a brick-red colour.

When protoxide of lead is substituted for black oxide of copper, we obtain that protoxide crystallized in small cubes or square plates, provided the crucible be kept only a very short time at an incipient red heat. If the red heat be continued for some time, the protoxide of lead passes into the peroxide, which crystallizes in hexagonal plates of a flea-brown colour. Phosphate and sulphate of lead treated in the same way are completely decomposed, while crystals of protoxide and peroxide of lead are formed. By the same process the oxide of cobalt may be obtained in square plates, and the oxide of zinc in needles of a dirty yellow colour.

It has been often remarked, that in chemical operations the walls of the vessel have an influence on the reactions. M. Becquerel attempted to ascertain the nature of this influence.* He took a narrow glass tube, open at both extremities, and introduced by one end a quantity of calcined oxide of cobalt reduced with water into a very fine paste. The aperture was then closed, a silver wire introduced into the tube, and the upper portion of it filled with a solution of chloride of chromium. After a fortnight small dendritical metallic crystals began to be perceived on the inferior part, and on the surface of the tube. He obtained the same effect without employing the silver wire. The oxide of cobalt, which is not easily reduced by the action of Voltaic electricity, yields in this instance to a current apparently feeble, but which, however, must possess considerable energy.

The inner surface of the tube must have exercised a great influence, as the reduction took place above. In the slow action of the chloride of chromium on the oxide of cobalt,

* Ann. de Chim. et de Phys. lii. 243.

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electricity was disengaged. The two fluids must have united along the inner surface of the tube, which served to establish the poles of the small battery.

M. Becquerel's next attempt was to obtain sulphurets by other processes.* He experimented chiefly on sulphurets of lead and of antimony. Into a tube of glass, shut at one end, he put a quantity of sulphuret of mercury; over it was poured a solution of chloride of magnesium, and a metallic plate of lead was passed through the solution to the very bottom of the tube. The tube was now sealed hermetically, and its contents left to the slow action of the electric current induced by their mutual action on each other. In about six weeks a thin metallic film began to be perceived on the inner surface of the tube, just above the sulphuret of mercury. This film detached itself, and its inner side was studded with tetrahedral crystals of pure sulphuret of lead. On opening the tube, a gas was disengaged, having the odour belonging to combinations of sulphur with chlorine and hydrogen. Antimony subjected to a similar treatment, also furnished crystals; but their nature was not so accurately determined, though probably they were sulphuret of antimony.

Such are the most important of the experiments made by M. Becquerel to form a great number of combinations and decompositions by means of weak electric currents. At the meeting of the British Association held at Bristol in 1836, Andrew Crosse, Esq., read to the chemical section the result of a set of similar experiments, on which he had been engaged for several years. Without being aware of what had been previously done by M. Becquerel, Mr Crosse had obtained many similar results, and came to similar conclusions. It will be sufficient if we state here the general result of Mr Crosse's investigations.

Experiments of Mr Crosse.

In the neighbourhood of Broomfield, in Somersetshire, there is a cavern, the roof of which is partly studded with arragonite and calcareous spar in fine crystals. The water which flows from that roof contains in the quart about ten

* Ann. de Chim. et de Phys. liii. 105.

substances were upon which these currents acted, and what the specific changes induced.

The experiments of Mr Fox and Mr Henwood constitute an interesting commencement of these very important investigations. The veins of lead ore in the north of England, being so much more numerous, and less deep, than those of Cornwall, and passing through a greater number of beds, would deserve to be experimented on in a similar manner. Is it not probable that the thin layers of iron pyrites so common in pit-coal, owe their origin to electric currents?

Electricity employed to engrave in relief.

Mr Spencer of Liverpool has made a very ingenious application of a Voltaic battery of low intensity, which promises to be exceedingly useful by enabling us to form engravings on copper in relief, which may be substituted for wood-cuts, and thus printed at the same time with the letter-press. The method which he has suggested is this:—

Into the glass vessel A, pour a saturated solution of sulphate of copper. Place the glass cylinder B, open at both ends, but having the bottom shut with a plug of plaster of Paris about an inch thick, so that it shall occupy the centre of the vessel A. Fill it with a solution of common salt. C is the copper plate on which the engraving is to be made. A copper wire is to be soldered to it, while a plate of zinc is soldered to the other end of it. The wire is to be so bent that the copper plate C is just under the plaster plug, and of course immersed in the sulphate of copper solution; while the zinc plate is above the plaster plug in the small glass cylinder, and of course immersed in the solution of common salt.

The copper plate C, before being soldered to the wire, is covered with varnish, and the figures or lines wished to be formed upon it in relief are drawn upon the varnish, taking care that every line penetrates to the metal. After the graver has done its duty, pour upon the varnish a little dilute nitric acid, and allow it to remain till the oxide is completely

Part II. removed, and the bottom of every line consists of metallic copper. Now, place the whole as in the figure. The plaster of Paris plug becoming wetted with the liquid, allows the Voltaic energy to pass, and the circle is complete. Metallic copper is deposited on every line, and adheres firmly to the metallic plate, so that when the varnish is removed, every line is exhibited in relief. Thus the copper plate may be set up along with the letter-press, precisely like engravings upon wood.

This method will obviously save a good deal of expense, and may be applied in a great variety of ways. Mr Spencer has very ingeniously pointed out, or alluded to various purposes to which it may be applied. He has shown how facsimiles of coins, medals, &c., may be made, and how engravings may be multiplied by it *ad infinitum.* This must be particularly useful in the potteries, where ten or twelve engravings of the same pattern are often required.

Mr Spencer's pamphlet has been just printed in Liverpool. It is entitled, *An Account of some Experiments made for the purpose of ascertaining how far Voltaic Electricity may be usefully applied to the purpose of Working in Metal. Being a paper read at a meeting of the Liverpool Polytechnic Society, held at the Medical Institution, Mount Pleasant, on Thursday, the* 12*th September*, 1839. It is a most interesting pamphlet of twenty-six pages, and exhibits one of the happiest and most masterly pieces of inductive reasoning in the English language.

CHAPTER XIX.

OF THE ELECTRICITY OF THE ATMOSPHERE.

After discovering the Leyden phial, and witnessing the effects of electrical batteries, electricians could not avoid perceiving the analogy which existed between lightning and

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electricity. This analogy was accordingly pointed out by various individuals, especially by the Abbé Nollet; but it was Dr Franklin, who, having discovered the power of pointed bodies to discharge electricity at a distance, first thought of employing that method to render atmospherical electricity sensible. Not having in America the requisite means for such an experiment, he requested the electricians in Europe to turn their attention to it. The first person who attended to his suggestion was M. D'Abilard, who constructed an apparatus for the purpose at Marly la Ville, about 6 leagues from Paris. It consisted of an iron rod 40 feet long, the lower extremity of which was brought into a sentry-box where rain could not enter; while on the outside it was fastened to three wooden posts by silken strings defended from the rain. M. D'Abilard was obliged to leave home, but in his absence he intrusted his machine to one Coisier, a joiner, who had served 14 years among the dragoons, and on whose courage and understanding he could depend. This artisan had all the requisite instructions given him, and he was desired to call some of his neighbours, particularly the curate of the parish, whenever there should be any appearance of a thunder storm. On Wednesday the 10th of May, 1752, between two and three in the afternoon, Coisier heard a pretty loud clap of thunder. Immediately he ran to the machine, taking with him a phial furnished with a brass wire; and presenting the wire to the end of the rod, a small spark issued from it with a snap like that of a spark from an electrified conductor. Stronger sparks were afterwards drawn in the presence of the curate and a number of other people. The curate's account of them was that they were of a blue colour, an inch and a half in length, and smelled strongly of sulphur. In taking them, Coisier received a stroke on his arm, a little below the elbow.

Lightning and electricity the same.

Verified by D'Abilard.

Eight days after this, M. Delor witnessed the same appearances at his house situated in one of the highest spots in Paris. His apparatus was similar to that of D'Abilard, excepting that his iron rod was 99 feet in length. There was no thunder nor lightning, only a cloud passed over the house.

Dr Franklin, after having published his views on this sub-

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Franklin's electric kite.

ject, was waiting for the erection of a spire in Philadelphia to execute his experiment. It occurred to him that by means of a common kite, he could have a readier and better access to the regions of thunder, than by any spire whatever. Preparing therefore a large silk handkerchief, and two cross sticks of sufficient length on which to extend it, he took the opportunity of the first approaching thunder storm, to take a walk into a field in which there was a shade convenient for his purpose. But dreading the ridicule which too commonly attends unsuccessful attempts in science, he communicated his intended experiment to nobody but his son, who assisted him in raising the kite.

The kite being raised, a considerable time elapsed before there was any appearance of its being electrified. One very promising cloud had passed over it without effect; when at length, just as he was beginning to despair of his contrivance, the rain having wetted the string which was of hemp, and rendered it a better conductor, he observed some loose threads on it to stand erect, and to avoid one another, just as if they had been suspended on a common conductor. Struck with this promising appearance, he immediately presented his knuckle to the key which was tied to the end of the string, and the discovery was complete. He perceived a very evident electric spark. Others succeeded even before the string was fully wet, so as to put the matter past all dispute; and when the rain had wetted the string, he collected sparks very copiously. This happened in June, 1752; about a month after the French electricians had verified the theory, but before any information on the subject had reached Dr Franklin.

Hazard of the experiment.

This experiment of Franklin was attended with extreme danger. Had his cord been more moistened, or a better conductor than it was, it is probable that this celebrated man would have fallen a sacrifice to his temerity, and we would have been deprived of all the benefits which he afterwards conferred upon science, upon his own country, and upon mankind in general. This actually happened soon after to Professor Richmann of Petersburg, who was struck dead by a flash of lightning on the 6th of August, 1753, drawn by his

Death of Professor Richmann.

apparatus into his own room, as he was attending to an experiment which he was making with it.

He had provided himself with an instrument, which he called an electrical gnomon, the use of which was to measure the strength of electricity. It consisted of a rod of metal terminating in a small glass vessel, into which he had (for what reason does not appear,) put some brass filings. At the end of this rod a thread was fastened, which hung down by the side of the rod when it was not electrified, but when it was it avoided the rod, and stood at a distance from it, making an angle at the place where it was fastened. To measure this angle he had the arch of a quadrant fastened to the bottom of the iron rod.

He was observing the effect of the electricity of the clouds, at the approach of a thunder storm upon his gnomon, and of course standing with his head inclined towards it, accompanied by M. Solokow (an engraver, whom he frequently took with him to be a joint observer of his electrical experiments, in order to represent them the better in his figures), when this gentleman, who was standing close to his elbow, observed a globe of blue fire, as he called it, as big as his fist, jump from the rod of the gnomon, towards the head of the Professor, which, at that instant, was about a foot distant from the rod. This flash killed Mr Richmann; but Mr Solokow could give no account of the particular manner in which he was immediately affected by it. For at the same time that the Professor was struck, there arose a sort of steam or vapour, which entirely benumbed him, and made him sink down upon the ground; so that he could not remember even to have heard the clap of thunder, which was very loud.

The globe of fire was attended with a report as loud as that of a pistol. A wire which brought the electricity to the metal rod was broken to pieces, and its fragments thrown upon Mr Solokow's clothes. Half of the glass vessel in which the rod of the gnomon stood, was broken off, and the filings of metal that were in it were thrown about the room. The door-case of the room was half split through, the door torn off and thrown into the room. The shoe on the Professor's

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left foot was burst open, and there was a blue mark on the foot at that place. From which it was concluded, that the electricity had entered by the head, where there were evident marks of injury, and made its way out again by the left foot.*

Improvements in the kite.

M. de Romas made the experiment with the kite in a more perfect manner than the first attempt of Dr Franklin. He twisted a fine iron wire into the cord of the kite.† To prevent the observer from being exposed to danger, the lower extremity of the string terminated in a silk cord eight or ten feet in length, by means of which the kite with its string was insulated. Instead of drawing sparks with the finger, which makes the observer himself receive the charge, he received them by means of a metallic conductor, connected with the ground by a chain, which he held in his hand by means of an insulating glass handle, so that it resembled our common discharger. Romas describes the sparks given out from the string to this discharger during a thunder storm, in a letter to the Abbé Nollet, in very glowing language. "Conceive," says he, "plates of flame nine or ten feet long, and an inch thick, which made as much noise as a pistol. In less than an hour I had certainly thirty plates of this size, without reckoning a thousand others of seven feet, and below that. But what gave me the greatest satisfaction in this new spectacle, was, that the greatest of these plates were spontaneous, and that notwithstanding the abundance of the fire which they contained, they fell always on the nearest conductor. This

* See Phil. Trans. 1754, vol. xlviii. p. 765. Vol. xlix. p. 61. Professor Richmann was born in Livonia, on the 11th July, 1711, his father, William Richmann, having died of the plague before the birth of his son. He was educated at Revel, Halle, and Jena, and devoted himself to mathematics and physics. He went to Petersburg as tutor to the children of Count Ostermann. In 1735, he became an adjunct of the Academy. In 1741, he was made extraordinary Professor of Physics, and in 1745, at the death of Krafft, he became ordinary Professor of Physics. Though his papers are valuable, his death has given him a greater degree of celebrity than he would otherwise probably have attained. See Novi Comment. Acad. Petropol. tom. iv. for 1752–3, p. 36.

† Mr Cuthbertson employed a brass wire which is better, and M. Charles used a cord all of metal threads.

constancy gave me so much security, that I was not afraid to draw sparks by means of my conductor, even when the thunder storm was at its height, although the glass handle of that instrument was only two feet in length. I conducted where I pleased without feeling in my hand the smallest commotion, sparks of fire six or seven feet long, with the same facility as those whose length did not exceed seven or eight inches."

I refer those readers, who may wish to employ an electrical kite for similar purposes, to the late Mr Cuthbertson's *Practical Electricity*, p. 237, where they will find the contrivances which he employed to secure the observer against all risk of danger. It may, however, be proper to observe, that the experimenter should never hold the string of the kite in his hand, even though the lower part of it be of silk, in case rain or any accidental cause should make it a conductor. The tremendous size of the sparks described above by Romas, shows that in such a case they might produce fatal effects.

Intensity of electricity at the top of a rod.

Indeed, it is easy to see how intense the electricity may become at the end of the cord, even when the thunder clouds are not very formidable in appearance. Poisson has demonstrated that in the case of an insulated cylinder of small dimensions in the neighbourhood of a charged conductor, the thickness of the coating of electricity, on the cylinder is inversely, as its diameter compared to the osculating sphere of the charged conductor. Let $r =$ diameter of the cord of the kite, $\mathrm{R} =$ diameter of the osculating sphere of the thunder cloud, $\mathrm{E} =$ thickness of the coating of electricity of the thunder cloud, e that of the string, then it has been demonstrated that $e = m\,\frac{\mathrm{E}\,\mathrm{R}}{r}$, m being a constant coefficient, which experiment has shown to be very nearly $\frac{9}{48}$. But the thickness of the coating at the extremity of the cylinder, being to that at any other part, as 2·3 to 1, the expression for it will be $\frac{20{\cdot}7}{48} \cdot \frac{\mathrm{E}\,\mathrm{R}}{r}$.

To apply this expression to the thickness of the coating of electricity at the end of the kite string, let us suppose the

Part II. diameter of the string to be one line, while the radius of the osculating circle of the cloud is 1000 feet. Then $R =$ 144,000 lines, and $r = 1$. The expression becomes

$$\frac{20\cdot7}{48} \cdot \frac{144,000\ E}{1} = 62,100\ E.$$

Thus it appears that the thickness of the coating of electricity at the extremity of the cord of the kite, is no less than 62,100 times greater than upon the charged cloud. Now, as the pressure on the air is equal to the square of the thickness, it will be almost 400 million times greater at the end of the string, than on the surface of the cloud. We need not be surprised then that such terrible sparks issue from the end of the string, even when the thunder cloud is not very considerable.

Thus, it has been demonstrated by the sagacity of Dr Franklin, that *thunder and lightning* is merely a case of electrical discharges from one portion of the atmosphere to another, or from one cloud to another. Air, and all gases, are non-conductors; but vapour and clouds, which are composed of it, are conductors. Clouds consist of small hollow bladders of vapour, charged each with the same kind of electricity. It is this electric charge which prevents the vesicles from uniting together, and falling down in the form of rain. Even the vesicular form which the vapour assumes, is probably owing to the particles being charged with electricity. The mutual repulsion of the electric particles may be considered as sufficient (since they are prevented from leaving the vesicle by the action of the surrounding air, and of the surrounding vesicles,) to give the vapour the vesicular form.

Formation of clouds.

In what way these clouds come to be charged with electricity, it is not easy to say. But as electricity is evolved during the act of evaporation,* the probability is, that clouds are

* M. Pouillet has published a set of experiments, which seem to overturn Volta's theory of the evolution of electricity by evaporation. He has shown that no electricity is evolved by evaporation, unless some chemical combination takes place at the same time. But it follows from his experiments that electricity is evolved abundantly during combustion. The burning body giving out resinous, and the oxygen vitreous electricity. In like manner, the carbonic acid emitted by vegetables is charged

always charged with electricity, and that they owe their existence, or at least their form, to that fluid. It is very probable that when two currents of dry air are moving different ways, the friction of the two surfaces may evolve electricity. Should these currents be of different temperatures, a portion of the vapour which they always contain will be deposited; the electricity evolved will be taken up by that vapour, and will cause it to assume the vesicular state constituting a cloud. Thus we can see in general how clouds come to be formed, and how they contain electricity. This electricity may be either vitreous or resinous according to circumstances. And it is conceivable, that by long-continued opposite currents of air, the charge accumulated in a cloud may be considerable. Now, when two clouds charged, the one with positive, and the other with negative, electricity, happen to approach within a certain distance, the thickness of the coating of electricity increases on the two sides of the clouds which are nearest each other. This accumulation of thickness soon becomes so great as to overcome the pressure of the atmosphere, and a discharge takes place, which occasions the flash of lightning. The noise accompanying the discharge constitutes the thunder clap, the long continuance of which partly depends upon the reverberations from neighbouring objects. It is therefore loudest and largest, and most tremendous, in hilly countries.

Description of a thunder storm.

A thunder storm in this country commonly commences in the following manner:—A low dense cloud begins to form in a part of the atmosphere that was previously clear. This cloud increases fast, but only from its upper part, and spreads into an arched form, appearing like a large heap of cotton wool. Its under surface is level, as if it rested on a smooth plane. The wind is hushed, and every thing appears preternaturally calm and still.

Numberless small ragged clouds, like teazled flakes of

with resinous electricity, and the oxygen (probably) charged with vitreous electricity. These two sources are sufficiently abundant to account for the vast quantity of electricity so often accumulated in the clouds. See Ann. de Chim. et de Phys. xxxv. 401.

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cotton, soon begin to make their appearance, moving about in various directions, and perpetually changing their irregular surface, appearing to increase by gradual accumulation. As they move about they approach each other, and appear to stretch out their ragged arms towards each other. They do not often come in contact; but after approaching very near each other, they evidently recede either in whole, or by bending away their ragged arms.

During this confused motion, the whole mass of small clouds approaches the great one above it; and when near it, the clouds of the lower mass frequently coalesce with each other, before they coalesce with the upper cloud. But as frequently the upper cloud coalesces without them. Its lower surface, from being level and smooth, now becomes ragged, and its tatters stretch down towards the others, and long arms are extended towards the ground. The heavens now darken apace, the whole mass sinks down; wind rises and frequently shifts in squalls; small clouds move swiftly in various directions; lightning darts from cloud to cloud. A spark is sometimes seen coexistent through a vast horizontal extent, of a zigzag shape, and of different brilliancy in different parts. Lightning strikes between the clouds and the earth—frequently in two places at once. A very heavy rain falls—the cloud is dissipated, or it rises high and becomes light and thin.*

These electrical discharges obviously dissipate the electricity, the cloud condenses into water, and occasions the sudden and heavy rain which always terminates a thunder storm. The previous motions of the clouds, which act like electrometers, indicate the electrical state of different parts of the atmosphere.

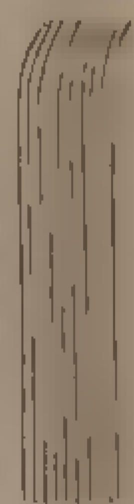
Explanation of the phenomena.

Thunder then only takes place when the different strata of air are in different electrical states. The clouds interposed between these strata, are also electrical, and owe their vesicular nature to that electricity. They are also conductors. Hence they interpose themselves between strata in different

* Robison; Supplement to third Edition of the Encyclopædia Britannica, vol. ii. p. 681.

states, and arrange themselves in such a manner as to occasion the mutual discharge of the strata in opposite states. The equilibrium is restored, the clouds deprived of their electricity collapse into rain, and the thunder terminates.

In thunder storms, the discharges usually take place between two strata of air, very seldom between the air and the earth. But that they are sometimes also between clouds and the earth, cannot be doubted. I once witnessed a thunder storm from the summit of Lochnagar, a very high granite mountain in Aberdeenshire. The thunder was at a great distance east. The first clap was nearly due east, distant not less than ten or twelve miles. The second was north-east, and equally far off. Thick black clouds intervened between me and the thunder, so that I saw no lightning. After watching the progress of the storm for about half an hour, suddenly a white cloud of a very peculiar appearance stretched itself between the part of the heavens where the thunder was, and the earth. This cloud was composed of distinct parallel fibres bent as in the margin: it continued about half an hour, during which, it conveyed away all the surplus electricity from the clouds to the earth. For no more thunder was heard, and the clouds discharged themselves in a heavy shower of rain, which terminated the thunder storm.



Electrical discharges without noise.

These electrical discharges sometimes take place without any noise. In that case the flashes are very bright, but they are single flashes passing visibly from one cloud to another, and confined usually to a single quarter of the heavens. When they are accompanied by the noise, which we call *thunder*, a number of simultaneous flashes, of different colours, and constituting an interrupted zigzag line, may generally be observed stretching to an extent of several miles. These seem to be occasioned by a number of successive or almost simultaneous discharges from one cloud to another; these intermediate clouds serving as intermediate conductors, or stepping-stones for the electrical fluid. It is these simultaneous discharges which occasions the rattling noise, which we call thunder.

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Though they are all made at the same time, yet as their distances are different, they only reach our ear in succession, and thus occasion the lengthened rumbling noise so different from the snap, which accompanies the discharge of a Leyden jar.

Nature of the thunder clap.

If the electricity were confined to the clouds, a single discharge (or a single flash of lightning) would restore the equilibrium. The cloud would collapse and discharge itself in rain, and the serenity of the heavens would be restored. But this is seldom the case. I have witnessed the most vivid discharges of lightning from one cloud to another, which enlightened the whole horizon, continue for several hours, and amounting to a very considerable number, not fewer certainly than fifty, and terminating at last in a violent thunder storm. We see that these discharges, though the quantity of electricity must have been immense, did not restore the equilibrium. It is obvious from this, that not only the clouds but the strata of air themselves, must have been strongly charged with electricity. The clouds being conductors served the purpose of discharging the electricity with which they were loaded, when they came within the striking distance. But the electric stratum of air with which the cloud was in contact, being a non-conductor, would not lose its electricity by the discharge of the cloud. It would immediately supply the cloud with which it was in contact with a new charge. And this repeated charging and discharging process would continue to go on till the different strata of excited air were brought to their natural state.

Electrical state of the atmosphere

From the atmospherical electric journal kept by Mr Read at Knightsbridge, during two whole years from the 9th of May, 1789, to the 9th of May, 1791, it appears that clouds, and rain, and hail, and snow, are always charged with electricity; sometimes with negative, but more frequently with positive electricity. When the sky is serene and cloudless, the strata of air are generally charged with positive electricity. In such cases the thunder rod is charged by induction; the highest end acquiring the opposite state of electricity from the air, and the lowest end the same kind of electricity, while a portion of the rod towards the middle is neutral. During the

first year, there occurred only seven days in which no electricity could be perceived. But during the second year, when the apparatus was much more complete, not a single day occurred which did not give indications of electricity in the atmosphere.

During the first year, the electricity was positive 241 times, and during the second, 423 times. This difference was chiefly owing to the apparatus. During the first year, there occurred seventy-three days in which the signs of electricity were so weak, that the kind could not be determined; and there were seven days in which no signs of electricity at all were perceptible. But during the second year, the apparatus was so much more perfect, that no day occurred without signs of electricity; and it was ascertained, that on those days in which the electricity is weak, it is always positive. During the first year, the electricity was observed negative 156 times, and during the second year, 157 times. During the first year, sparks could be drawn from the apparatus during ninety-eight days, and in the second year, during 106 days. From these facts, the probability is, that the electrical state of the atmosphere did not differ much in each of the two years, during which the observations were kept.

Observations in Germany.

The observations of Read have been prosecuted in Italy and Germany, where, during the summer season, the air is generally much drier than in Great Britain, and therefore, better fitted for electrical observations. The general result is as follows:—

During fine weather, the atmosphere always contains an excess of positive electricity. This excess is very small a little before sunrise. It augments as the sun rises, at first very slowly, and afterwards more rapidly, and reaches its maximum some hours after sunrise. It then begins to diminish at first rapidly, then slowly, and reaches its minimum a few hours before sunset. It begins to augment as soon as the sun begins to approach the horizon, and a few hours after, reaches a second maximum: then it diminishes till toward the rising of the sun, when the same alternations are repeated.

Schubler, by his experiments and observations, has given

Part II. greater precision to these general statements.* His ex
ments are too numerous, and occupy too much room to pe
us to insert them all here. Very copious extracts from t
have been copied by Becquerel into the fourth volume of
Electricity (p. 85). The following table exhibits the resu
hourly observations made on the intensity of the electrici
the atmosphere, at Stuttgard, on the 12th of May, 1811.

Hour.	Electrometer.	Saussure's Hygrometer.	Thermometer Centigrade.	Weather.
Morning.				
4 o'Clock,	+ 5	88°	9°·3	
5 . .	+ 6½	88	9·5	Quite serene.
6 . .	+ 8	87	10·5	horizon became g
7 . .	+11	86	12·1	ually foggy. Dew
8 . .	+13	84	13·5	
9 . .	+10	76	15·6	
10 . .	+ 8	70	17	The horizon
Noon, .	+ 7	63	20·1	came clear, and
2 P.M. .	+ 6½	61	21·6	sky assumed a p
4 . .	+ 5½	60	21·3	blue colour.
5 . .	+ 5	62	20·9	
6 . .	+ 6	65	20·0	Vapours again m
7½ . .	+ 8	72	17·5	their appearance.
8½ . .	+12	83	15·5	Evening, dew fel
9½ . .	+ 8	86	13	
10½ . .	+ 7	88	12·1	Quite serene.
Midnight,	+ 6½	88	11	
2 A.M. .	+ 5½	88	10·1	

From these observations we see, that at Stuttgard, on
12th of May, 1811, the positive electricity of the atmosph
was at a maximum at eight in the morning. It became g
dually feebler and feebler, and was at its minimum at five, r
From that time it became stronger, and reached a seco
maximum at half-past eight, P.M. From this time it s
again, and reached a second minimum about two in the mo
ing, when Schubler discontinued his observations. Fr
other observations of Schubler, which we cannot find room
here, it appears that the maxima and minima show occasion
some little differences in the time when they occur. Th

* Schweigger's Jour. iii. 123, 268.

for example, in the beginning of June, 1811, the maxima took place most frequently about seven, A.M., and nine or half-past nine, P.M.; while, towards the end of that month, they occurred at six, A.M., and ten, P.M. In the first half of July, the electrical periods were not very distinct, owing to the state of the atmosphere. In the last half of that month, the two maxima occurred at seven, A.M., and nine or ten, P.M. According to Schubler, the electric periods were most distinct in those days which were dry and hot, and on which luminous striæ, analogous to the aurora borealis were perceptible in the sky. Ch. XIX.

During the first ten days of August, the weather was so bad, that observations could not be made. During the last two-thirds of the month, the weather was clearer, the maxima were at eight, A.M., and half-past eight, P.M.

In September, the maxima corresponded usually to half-past eight, A.M., and half-past seven or eight, P.M.

In October the maxima occurred at seven, A.M., and seven, P.M., and the minima at two, P.M., and ten, P.M.

In November, the maxima were at nine, A.M., and half-past six, P.M., and the minima at half past-seven, A.M., and half-past ten, P.M.

Schubler's observations confirmed an opinion that had been long entertained, namely, that the force of electricity for the two maxima and two minima, goes on increasing from the month of July to the month of January, inclusive. So that the greatest intensity happens in winter, and the weakest in summer: hence we find that in serene days of winter the augmentation of the electricity is always proportional to the increase of the cold. From the mean of the months it appears that the first minimum and the first maximum have less intensity than the second minimum and second maximum.

In Paris. M. Arago has made a careful set of observations on the electricity of the atmosphere, at the Paris observatory. He has confirmed the occurrence of two maxima and two minima, every twenty-four hours.*

* See a set of [illegible] observations inserted by Becquerel in his *Traité d'Electricité*, iv. 93.

Part II.

From the observations of Schubler, it appears that when the sky is cloudy, the electricity of the atmosphere is still positive, and that its intensity is greater in winter than in summer; that during storms, or when it rains or snows, the electricity is sometimes positive and sometimes negative, and that its intensity is much more considerable than during serene weather. Observations were made upon 140 rainy or snowy days; during seventy-one of these days, the electricity of the atmosphere was positive, and during sixty-nine days it was negative.

Arago observed, that during the first seven months of 1830, there were twenty-five rainy days, eleven of these days showed no signs of electricity in the atmosphere; during six of them, the electricity was positive, and during eight, it was negative.

The electricity of the atmosphere sometimes changes its state several times in the same day. Thus Arago remarked, that on the 19th of April, 1830, which was a rainy day, the electricity was five times negative and three times positive; and that on the 11th of May, the negative electricity continued for some time after the cessation of the rain. Volta observed no fewer than fourteen changes of state in the electricity of the atmosphere during a storm. Similar observations were made by Saussure.

Saussure observed farther, that in summer when serene weather follows several rainy days, and while the earth is still covered with humidity, the diurnal period of the electrical intensity of the atmosphere resembles that of winter. This points out the cause why the intensity of the electricity of the atmosphere is greater in winter than in summer.

Electrical state in the open country.

The preceding observations apply to the results obtained in observatories and buildings, and not to the open country and to mountainous districts, where local causes have a great influence on the excess of free electricity found in the atmosphere. From the observations of Saussure, it appears that the electricity of mountainous and isolated places has more intensity; that it is slight, if any, in houses, under trees, and in streets. It is sensible in wide streets and squares, and particularly in bridges. About Geneva, it appears, from

Saussure's observations, that the intensity of the electricity of the atmosphere is greatest during fogs which do not resolve themselves into rain, nor rise to a great height above the surface of the earth.

Chemical decompositions may go on in the atmosphere.

Since the atmosphere usually contains a notable quantity of free electricity, there can be no doubt that electrical currents may be readily induced in it, and that the numerous chemical decompositions which have been already described as produced by its powerful agencies, may frequently take place. This has been shown by an experiment of Mr Alexander Barry, so interesting that it deserves to be described.* In August, 1824, he elevated an electric kite in an atmosphere favourable to the exhibition of electrical phenomena. It was raised from an apparatus firmly fixed in the earth, and was insulated by a glass pillar. The usual shocks were felt on touching the string. The portion of string let out, with a double gilt thread passed through it, was about 500 yards. Mr Barry took two glass tubes, shut at the upper extremity, but having a platinum wire passing through the shut end, and reaching as far as the middle of the tube. The tubes were filled with a solution of sulphate of soda, tinged blue by syrup of violets, and placed inverted in two small glass cups filled with the same solution. These two cups were united by means of a bent glass tube, filled with the same solution, and dipping into each cup. We shall call the two inverted glass tubes A and B. The platinum wire in A was connected with the string of the kite by a piece of gilt thread, while the wire in B, by a similar thread, was connected with the earth. It is clear that in this case the direction of the current was from A to B; so that the wire in A was positive, and the wire in B negative. Soda was speedily accumulated round the pole A, as became evident by the green colour which the syrup of violets in the tube assumed. Sulphuric acid was evolved round the wire of B, and the consequence was, that the liquid in the tube B became red.

* Phil. Trans. 1831, p. 165.

CHAPTER XX.

OF ANIMAL ELECTRICITY.

THERE are four species of fish that possess a curious elect cal apparatus, which enables them to give shocks, and benumb and seize their prey. These are the *raia torpe* *gymnotus electricus*, *tetrodon electricus*, and *silurus electric* The torpedo is a native of the European seas, but seems thrive best in the Mediterranean. Its power of benumbi the hands of those persons who touched it, was known to t ancients; and the name *torpedo*, given it by the Romans, w derived from that property, though the science of electrici had made too little progress to enable them to assign this e traordinary power to the right cause.

Torpedo. 1. The *raia torpedo* belongs to the order of cartilagino fishes. The body is of a somewhat circular form, sligh convex above; about 18 inches or 2 feet in length, and f the most part, of a pale-reddish brown on the upper surfa sometimes marked by five large and dusky spots, and whit or flesh-coloured beneath.†

History. The first attempt to give an anatomical description of t structure of the torpedo, was made by Stephano Lorenzi in 1678.‡ The benumbing faculty, according to him, is seat in the two semi-circular or falcated muscles on each si of the thorax, which consist of fibres, irregular, but as lar as a goose quill, and made up of bladders filled with a ki of water. One end of these fibres is fixed to the skin of t belly, and the other end to that of the back. When the f contracts those fibres, there issue out corpuscles fitted to t pores of a man's skin, so as to enter into immediate conta

* Plinii Natur. Hist. lib. xxxii. c. 1.

† See a figure of the torpedo by Mr Walsh, in Phil. Trans. 1773 461.

‡ Philos. Collect. i. p. 42.

and disturb the posture of the parts, and to cause pain as when one's elbow is hit or knocked. This mechanical explanation of Lorenzini was afterwards advanced by Reaumur. Ch. XX.

Mr Walsh, in a set of experiments made upon torpedos at Rochelle, in 1772, first proved that the shocks given by the torpedo were electrical discharges. They passed through all conductors, and were intercepted by all non-conductors.* Mr Walsh furnished Mr John Hunter with different torpedos, that he might be able to investigate the structure of the electrical organs of these animals. This he did in a paper published in the same volume of the *Philosophical Transactions* (p. 481). The electric organs of the torpedo are placed on each side of the cranium and gills, reaching from thence to the semi-circular cartilages of each great fin, and extending longitudinally from the anterior extremity of the animal, to the transverse cartilage which divides the thorax from the abdomen. Each organ is attached to the surrounding parts by a close cellular membrane, and also by short and strong tendinous fibres, which pass directly across from its outer edge to the semi-circular cartilages. Each electrical organ is about 5 inches in length, and at the anterior end, 3 inches in breadth, though it is but little more than half as broad at the posterior extremity. Each consists wholly of perpendicular columns, reaching from the upper to the under surface of the body, where they are placed. The longest column being about an inch and a half, the shortest about $\frac{1}{4}$ of an inch in length, and their diameters about $\frac{1}{5}$ of an inch. Its electric organs.

The figures of the columns are very irregular, varying according to situation, and other circumstances. The greater number of them are either irregular hexagons or pentagons; some, however, are pretty regular quadrangulars. Those of the exterior row are either quadrangular or hexagonal, having one side external, two lateral, and either one or two internal. In the second row they are mostly pentagons. Their coats are very thin, and seem transparent, closely connected with each other, having a kind of loose net-work of tendinous fibres

* Phil. Trans. 1773, p. 461.

Part II. passing transversely and obliquely between the columns, and uniting them more firmly together. These are mostly observable where the large trunks of the nerves pass; the columns are also attached by strong inelastic fibres passing directly from the one to the other.

The number of these columns varies with the size of the torpedo. In one of the organs of a very large torpedo, Mr Hunter reckoned 1182 columns; in those of an ordinary size, they amount to about 470. They increase not only in size, but in number, during the growth of the animal, new ones forming perhaps every year on the exterior edge, as there they are much the smallest. Each column is divided by horizontal partitions placed over each other, at very small distances, and forming numerous interstices, which appear to contain a fluid. These partitions consist of a very thin membrane, considerably transparent. Their edges appear to be attached to one another, and the whole is attached by a fine cellular membrane to the inside of the columns.

The number of partitions contained in a column of one inch in length, was found to amount to 150. The partitions are very vascular; the arteries are branches from the veins of the gills, which convey the blood that has received the influence of respiration. They pass along with the nerves to the electric organ, and enter with them; they then ramify in every direction. The nerves inserted into each electric organ arise by three very large trunks from the lateral and posterior part of the brain. Having entered the organs, they ramify in every direction between the columns, and send in small branches on each partition, where they are lost. The magnitude and number of these nerves is very great indeed.

Such is the anatomical structure of the electric organs of the torpedo, as determined by Mr Hunter. Some anatomical observations on these organs were afterwards made by M. Geoffroy Saint-Hilaire.* According to him, they consist of a great number of aponeurotic tubes filled with a substance of a peculiar nature. Volta, in a long paper on galvanic

* Jour. de Phys. lvi. 241.

electricity, which he published in 1800,* endeavoured to show, that these electric organs were similar in principle to the Voltaic pile.

Their chemical nature.

With respect to the chemical nature of the electrical organs of the torpedo, several interesting particulars have been ascertained by Dr John Davy.† The specific gravity, including the upper and under boundary skin, he found 1·026, while the specific gravity of a portion of the abdominal muscles of the same full-grown fish, was 1·058, and that of the thick strong muscles of the back, close to the spine, 1·065. The weight of the electrical organs in a torpedo, eight inches long and five inches across the widest part of the body, was 302 grains; the weight of the whole fish was 2065 grains. But when the electrical organs were dried by the heat of a steam bath, the weight was reduced to 22 grains. Supposing the matter dissipated to have been water, the electrical organs of the torpedo are composed of

Solid matter . .	22 or	7·28
Water . . .	280 or	92·72
	302	100·00

It would appear from this, that the greater part by far of the electric organs is water, doubtless holding in solution various substances to which the conducting power of the organs is to be ascribed.

When the electric organs are cut into small pieces, and placed in a funnel, a liquid makes its escape slightly turbid, of a very light fawn colour, and just perceptibly acrid. This liquid did not change the colour of turmeric or litmus paper. A cloudiness was occasioned by dropping into it a solution of nitrate of silver, which was not completely re-dissolved by ammonia. It was copiously precipitated by acetate of lead, and a cloudiness was produced by dropping into it nitrate of barytes and corrosive sublimate. By evaporation, it afforded a residue which deliquesced partially on exposure to a moist atmosphere, and had an acrid and bitter saline taste.

* Ann. de Chim. xl. 255. † Phil. Trans. 1832, p. 259.

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When the electric organs of the torpedo are immersed in boiling water, they suddenly contract in all their dimensions, and the columns became circular. When the fish was newly caught, the tendinous fibres of these pillars, after a few seconds' immersion in boiling water, were converted into jelly, and the columns falling asunder assumed the appearance and consistence of a translucent very soft mucilage. From these properties ascertained by Dr Davy, and from their not contracting by Voltaic electricity, it is obvious that the electric organs of the torpedo are not muscular; their walls seem to consist of membranes of a cartilaginous nature, filled with a liquid containing in solution animal and saline substances, doubtless differing in its nature in the different cells into which the pillars are divided, so as to constitute a Voltaic battery, in which liquids are substituted for metallic plates.

Dr Davy confirmed the experiments of Mr Walsh, who pointed out the analogy between the electric organs of the torpedo and a charged Leyden phial. The quantity of electricity appears to be great, but its intensity low: hence, it gives shocks, but the current is stopped by very feeble obstacles. Mr Walsh found that the thinnest possible stratum of air interposed effectually stopped the current; but Dr Davy, whose test of the presence of an electric current was more delicate, found that it could traverse, though not very well, a thin current of air. The electricity of the torpedo gives no light nor spark; but Dr Davy found it capable of communicating magnetism to needles and small bars of steel. It acted also on the electro-multiplier precisely as Voltaic electricity does; and Dr Davy succeeded in decomposing water by means of the electricity of the torpedo, and thus has shown that it is identical in its effects with Voltaic electricity.

Gymnotus electricus.

2. The *gymnotus electricus*, or electric eel, belongs to the order of apodal fishes, and is a native of the rivers in South America, and Africa.

History.

Its electrical properties were first made known by M. Richer, who was sent by the Royal Academy of Sciences of Paris, to make astronomical observations in Cayenne, in 1672.* "I was much surprised," he

* Mem. de l'Acad. Royal des Sciences, vii. 325

observed, "to see a fish, three or four feet long, resembling a large eel, which, on being touched, not only with the finger, but even with a stick, so benumbs the arm and the part of the body nearest the fish, that the individual remains for half-a-quarter of an hour without being able to move. Indeed, such a giddiness is produced, that the individual would fall down if he did not prevent this by putting himself in a horizontal position." M. Richer himself tried the fish, and found the effects such as above described. He informs us, that he did not know the name of the fish, and it is needless to say that he did not suspect that this power of the fish was owing to the discharge of electricity.

But Bancroft, in his *Essay on the Natural History of Guiana*, published in 1769, described this fish under the name of torporific eel as a native of the river Essequibo, and showed that the torporific effect was owing to an electric discharge, similar to that of a charged Leyden jar, which the animal could give at pleasure.

In 1775, Dr Williamson, of Philadelphia, made a set of experiments on this fish, which he distinguished by the name of gymnotus electricus, and showed, as Bancroft had already done, that its benumbing powers were the consequence of electric discharges.* A still more particular account of it was given at the same time by Dr Garden, of Charlestown.†

As soon as Mr Walsh had shown that the benumbing property of the torpedo was owing to electricity, the conclusion was irresistible that the gymnotus electricus possessed the same electrical powers. He procured specimens of the gymnotus, and satisfied himself that his conclusions were correct. To complete our knowledge of the electrical organs of this fish, he prevailed upon Mr John Hunter to dissect the animal, and describe the electric organs which it possesses.‡ They were described also with care and in considerable detail by Fahlberg, in 1801.§

Its electric organs.

The electrical organ constitutes more than one-third of the whole animal, and nearly one-half of that part of the flesh in

* Phil. Trans. 1775, p. 94. † Ibid. p. 102. ‡ Ibid. p. 395.

§ Kongl. Veten. Acad. Nya Handlingar, 1801, p. 122.

Part II. which it is placed. There are two pairs of these organs, a larger and a smaller, one being placed on each side. The large pair occupy the whole lower or anterior, and also the lateral part of the body, making the thickness of the fore or the lower parts of the animal, and run from the abdomen to near the end of the tail, or almost the whole length of the animal. These two organs are separated from one another at the upper part, by the muscles of the back, which keep their upper edges at a considerable distance from each other, and towards the middle they are separated by the air bag, and at their lowest parts they are separated by the middle partition. The general shape of the organ, on an external or side view, is broad at the end next the head of the animal, becoming gradually narrower towards the tail, and ending there almost in a point.

The structure is extremely simple and regular, consisting of two parts, viz., flat partitions or septa, and cross divisions between them. The outer edges of these septa appear externally in parallel lines, nearly in the direction of the longitudinal axis of the body. These septa are thin membranes placed nearly parallel to one another. Their lengths are nearly in the direction of the longitudinal axis, and their breadth is nearly the semi-diameter of the body of the animal. They are of different lengths, some being as long as the whole organ. They are in general broadest near the anterior end, answering to the thickest part of the organ, and become gradually narrower towards the tail. They have an outer and inner edge. The outer is attached to the skin of the animal, to the lateral muscles of the fin, and to the membrane which divides the great organ from the small; and the whole of the inner edges are fixed to the middle partition, to the air bladder, and three or four terminate on that surface which encloses the muscles of the back.

These septa on the side next the muscles of the back, are hollow from edge to edge, answering to the shape of these muscles, but become less and less so towards the middle of the organ. The distances between these septa differ in fishes of different sizes. In a fish of two feet four inches in length,

they were about $\frac{1}{27}$th of an inch distant from one another, and the breadth of the whole organ at the broadest part, about an inch and a quarter, in which space there were thirty-four septa. The small organ has the same kind of septa, in length passing from end to end of the organs, and in breadth passing quite across; they run somewhat serpentine. They differ very much in breadth from one another, the broadest being equal to one side of the triangle, and the narrowest scarcely broader than the point or edge. They are pretty nearly at equal distances from one another, but much nearer than those of the large organs, being only about $\frac{1}{30}$th of an inch asunder. But they are at a greater distance from one another towards the tail, in proportion to the increase of the breadth of the organ. The organ is about half an inch in breadth, and has fourteen septa.

These septa in both organs are very tender in consistence, being easily torn. They appear to answer the same purpose as the columns in the torpedo, and are to be considered as making so many distinct organs. These septa are intersected transversely by very thin plates or membranes, whose breadth is the distance between any two septa, and, therefore, of different breadths in different parts—broadest at the edge which is next the skin, narrowest at that next to the centre of the body. Their lengths are equal to the breadth of the septa between which they are situated. There is a regular series of them continued from one end of any two septa to the other; they appear to be so close as even to touch. In an inch in length, there are about 240.

The nerves supplying these organs are from the medulla spinalis. They are large, but not so much so as those of the torpedo. These fish have no teeth; but strike the small fishes on which they live into a state of torpor by an electric shock, and then swallow them. Humboldt, in his *View of the Equatorial Regions*, informs us that they abound in the rivers and lakes of Venezuelas and Caraccas. He went to a village in the neighbourhood of one of these lakes. The Indians drove about thirty mules into the pond. They were immediately attacked by the gymnoti. Two were drawn under

the water and drowned; the rest, driven frantic, made every effort to escape. By degrees the vigour of the fish was exhausted; their shocks became less frequent and less violent, and the mules recovered and made their escape. The exhausted fish were then easily taken.

A gymnotus, recently brought to London by Mr Porter, was purchased by the proprietors of the gallery in Adelaide street. Mr Faraday was invited to make experiments on this fish, and was enabled to ascertain the identity of its power with that of common electricity. It gave shocks, affected the galvanometer, converted a steel needle into a magnet, decomposed iodide of potassium, evolved heat, gave a sensible spark, and produced all the ordinary phenomena of electricity. It killed small fish by passing a powerful electrical shock through them.*

Tetrodon electricus.

3. The *tetrodon electricus* was first observed by Lieutenant William Paterson, in the island of Johanna, one of the Comoro islands, lying between the north-west of Madagascar and Africa.† It is seven inches long, two inches and a half broad, and has a long projecting mouth. The back of the fish is a dark brown colour, the belly part sea-green, the sides yellow, and the fins and tail of a sandy green. The body is interspersed with red, green, and white spots; the white ones particularly bright. The eyes are large, the iris red, its outer edge tinged with yellow.

These fishes were found in the hollows of coral rocks where the temperature of the sea water was between 56° and 60°. When Mr Paterson attempted to take one of these fish into his hand, it gave him so severe an electric shock that he was obliged to quit his hold. The electrical powers of this fish were experienced by different persons; but I am not aware of any attempts hitherto made to ascertain the structure of its electrical organ.

Silurus electricus.

4. The *silurus electricus* is found in some rivers in Africa. It seems to have been first noticed by M. Adanson, who went to Senegal in 1738, and continued there during six years

* Phil. Trans. 1839. † Ibid. 1786, p. 382.

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assiduously occupied in investigating the natural history of that country. It was afterwards noticed by Forskal in his detached papers, which were published by Niebuhr in 1775. It was more particularly described by M. Brussonet under the name of Le Trembleur, in the *Memoirs of the French Academy for* 1782. It is about twenty inches long, very broad in the fore part, depressed, and of a cinereous colour, with some blackish spots towards the tail. When touched it gives an electrical shock, which is not so strong as those given by the torpedo and gymnotus. As well as all the other electrical fishes it is used as food.

Its organs.

It was dissected, and the structure of its electrical organs described by M. Geoffroy St Hilaire. Instead of being placed on each side of the head as in the torpedo, or in the inferior part of the tail as in the gymnotus, they extend round the silurus. They consist of aponeurotic and tendinous fibres united so as to form a net, so fine that the meshes of it cannot be discovered by the naked eye. The cells of this net are filled by a mucous matter. The electric organ is covered by a strong aponeurosis, which prevents it from communicating with the interior.*

Not identical with a Voltaic battery.

Although the electrical organs of these fishes have been considered as analogous to a Voltaic battery; yet it must be acknowledged that the resemblance has not been fully made out. They act only while the animal is alive, and no electric currents can be observed so soon as death has taken place. Galvani cut off longitudinally a portion of the body of a torpedo with one of the electric organs, leaving untouched and in its natural position the part of the animal containing the other electrical organ, together with the head. This last portion gave shocks, while it was impossible to obtain any from the other portion. The same observation was made by Mr Todd. Galvani found also that whenever the head of the torpedo was cut off, the electric organs ceased to give shocks. But after the heart was removed, the organs continued to give shocks till the brain was removed, when all electrical

* Phil. Mag. (first series,) xv. 126.

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effects were destroyed. Galvani removed the brain with so precautions as not to disturb the circulation of the blood, y he could not obtain the slightest shock. Every lesion of t brain proved fatal to the animal's power of giving shocks. Fr these facts it would appear that the electrical powers of t organs of the torpedo are connected with the nervous energ

Convulsions of muscles of a frog.

The original discovery of Galvani, which led to the num ous series of facts that has thrown so much light upon elect city and chemistry, was, that the hind legs of a frog ne killed were thrown into convulsions when touched by a scalp while sparks were drawn from an electrical machine in t neighbourhood. This experiment was repeated and vari by physiologists in every part of the civilized world, doubtl from the new light which it was expected to throw upon t action of the nerves in the living body. The laws by whi these convulsions are regulated, have been ascertained w considerable accuracy; but it can hardly be said that a additional light has been thrown upon the animal economy, that the mysterious action of the nerves is better understo than it was before the discovery of Galvani.

How prepared.

The hind legs of the frog are prepared for exhibiting the convulsions by removing the skin, and exposing the musc of the leg and the crural nerve. Every other part bei removed, so that the hind legs are joined to the body of t animal by the crural nerves alone. If a piece of zinc placed on the crural nerve, and a piece of copper or silver the muscles of the leg, the instant that these two metals a brought into contact the muscles are strongly convulse The convulsions speedily cease; but they are renewed eve time that the contact is broken or the circle completed.

If the crural nerve be laid bare, and the prepared leg the frog suspended by a string tied to the extremity of t nerve, if we take hold of the leg, and bring it in contact wi the nerve, convulsions take place. From this it appears th the nerve and the muscles of the leg are in two differe electrical states. And it has been ascertained that the ner in this case is plus, and the muscle minus.

It is well known that muscles have the property of contra

ing when irritated. Their contractibility is considered by physiologists as referrible to the action of the nerves. When a nerve is irritated, it does not contract; but the muscle into which it enters, and through which it ramifies, contracts much more violently than when the irritation is applied to itself.

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This power of contracting exists only while the parts retain their vital energy; but it continues some time after the death of the animal, and longer in cold-blooded than in hot-blooded animals.

Connected with the nervous energy.

If we irritate a nerve in a living animal with a sharp-pointed instrument, the corresponding muscle contracts. If we continue the irritation on the same point, the contraction of the muscle gradually ceases: but if we carry the irritation to another part of the nerve, it is again renewed. When the nerve is cut, if we irritate it below the section, the animal perceives no sensation, but the muscle immediately contracts. If we irritate above the section, the reverse effects are produced the animal has the sensation of pain, but the muscle does not contract.

When a nerve is tied, all the muscles supplied by it lose their sensibility and power of contracting.

From these facts it is evident that the cause which makes the muscles contract is transmitted by the nerves. Electricity merely acts like any other irritating cause.

It has been affirmed, that nerves are better conductors of electricity than any other animal substances; but this has not been proved by satisfactory experiments.

Notwithstanding the numerous experiments made upon the convulsions into which the muscles may be thrown immediately after death, it cannot be said that any discovery of importance has resulted from them. The only important fact was established by Dr Wilson Philip. He divided the eighth pair of nerves in a rabbit, and removed a portion of the nerve. The stomach immediately lost the power of converting the food taken into it into chyme. But when a current of electricity, by means of a Voltaic battery, was made to pass through the stomach, the power of digestion was restored, and the food converted into chyme, just as if the nerve had not been

Part II. divided. From this fact it follows that electricity is capable of supplying the place of the nervous energy in the stomach of a rabbit. Should electricity be found to supply the functions of other nerves as well as those of the stomach, it would go far to prove the similarity, if not the identity, of the nervous energy and electricity.

CHAPTER XXI.

ACTION OF ELECTRICITY ON VEGETABLES.

Though many experiments have been made, and much has been written on the action of electricity on vegetables, it must be confessed that little information has been hitherto acquired. Indeed, the aspect of the sky, and its electrical state, is too variable and uncertain in this country to make it easy to contrive a method of experimenting that would elucidate the subject. This accounts in some measure for the very contradictory statements that have been given by different electricians.

Electricity acts on plants as on inorganic matter.

It is obvious from numerous experiments that electricity acts on living plants precisely as it does on inanimate matter. It decomposes the saline constituents of the plant; the acids being accumulated round the positive pole, and the alkalies or bases round the negative pole. Davy, for example, employed a sprig of mint vegetating vigorously as a medium of communication between the two extremes of a Voltaic battery. Wires from these extremities were plunged into two cups of distilled water, and the cups were united by means of the sprig of mint dipping into each. In ten minutes potash and lime appeared in the negative cup, while an acid which precipitated chlorides of barium and calcium, and nitrate of silver, was found in the positive cup. This decomposing power is so strong that electricity has been employed as a means of analyzing vegetable bodies. Pelletier and Couerbe employed Voltaic electricity to show that picrotoxin possesses

the character of an acid. Picrotoxin combined with potash was subjected to the action of a Voltaic battery; in half an hour the picrotoxin was accumulated round the positive pole, and the potash round the negative pole.*

The consequence of this decomposing power, if the action be continued long or the battery be very powerful, may be the destruction of the vegetable so treated. Davy subjected a laurel leaf, during five days, to the action of a battery of one hundred and fifty pairs. It became brown. The chlorophyllite, resin, alkali, and lime, were transported to the negative pole, while the positive pole contained round it a quantity of hydrocyanic acid. The leaf appeared as if it had been exposed to the temperature of 500° or 600°.†

Negative electricity promotes germination.

There is an inconsistency in the experiments made to determine the effect of electricity on the germination of seeds. Bertholon and Ialabert found that the germination of seeds was accelerated by electricity. But Ingenhousz, having placed mustard seeds in grooves cut in cork, enveloped in blotting paper and swimming in a cup of water, placed the cup with its contents in a charged Leyden jar, and established a communication between the cups and the inside coating of the jar. The jar was kept constantly charged; yet the time which these seeds took to germinate was just the same as that of other seeds placed in the same manner, but not electrified.‡ It has, however, been shown that seeds in contact with the negative pole of a Voltaic battery, germinate better than seeds in contact with the positive pole. This follows from the numerous experiments of Becquerel. He employed only a single pair of plates. When the seeds were in contact with the copper plate they germinated sooner and more vigorously than when in contact with the zinc plate.§

Why.

Becquerel has shown that by the Voltaic action the salts contained in the seeds are decomposed, the alkali or base being accumulated round the negative, and the acid round the positive extremity: hence it follows that the presence of an alkali or base is favourable to germination, while the presence

* Ann. de Chim. et de Phys. liv. 181. † Phil. Trans. 1807, p. 52.
‡ Jour. de Phys. xxxv. 81. § Traité de l'Electricité, iv. 176.

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of an acid is unfavourable. Bulbs in this respect are precise the same as seeds. The result with seeds and bulbs is tl same, whether we employ a single pair of plates or a Volta battery.

Becquerel has succeeded in ascertaining the reason wl an alkali is favourable to germination. He has found l numerous experiments that during germination an acid given out by the seeds; and in the cases examined by hi the acid given out was the acetic. The same was the ca during the germination of bulbs, and likewise the developme of buds. This acid being thrown out, must of course be i jurious to germination: hence an alkali or base by saturati this acid, and of course removing it, must be favourable t germination.

Germinating seeds are negative.

It has been shown by Pouillet that when seeds germina they become negative. The reason is that germination analogous to combustion. When seeds germinate, they abso oxygen gas from the atmosphere, and emit carbonic aci Now, the carbonic acid evolved is positive, just as it is whe charcoal is burnt. The consequence of this must be that tl seeds become negative.

We have seen in the last chapter, that the excitability the muscles of animals by electricity, was owing to the nerv with which these muscles are supplied. As plants have noth analogous to nerves, few of them can be excited by electric

Electricity affects the sensitive plant.

The sensitive plant, the *mimosa pudica* and *sensitiva*, has t property of shutting up its leaves when irritated. Th the nearest approach to muscular action known to exist in t vegetable kingdom. Now, Giulio found that when Volta electricity was made to act upon the branches of these plan the leaves were shut up precisely as when they are irritate

It is obvious that the liquids which exist in vegetable s stances are not all of the same nature. The sap, for instanc before it undergoes digestion in the leaves, is very differe from the true sap, or *succus proprius*, contained in pecul vessels in the bark. This diversity, separated as the liqu

* Becquerel, Traite de l'Electricite, iv 162

e in plants by peculiar membranes, must occasion different Ch. XXI.
ctric states, and of course produce different electric cur-
its when the circuit is completed. This is well exempli-
l in a beautiful experiment, for which we are indebted to
Donné.* He introduced one of the platinum wires of an
ctro-multiplier into that part of a fruit which is in contact
h the stalk, and the other into the opposite extremity of
fruit. The magnetic needle moved 15°, 20°, or 25°,
cording to the fruit. In apples and pears the part conti-
ous to the stalk was plus, and the eye of the fruit minus.
the peach, apricot, and plum, it was the reverse.

* Becquerel, Traité de l'Electricité, iv. 164.

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THE END.

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